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THE PSYCHOLOGICAL REVIEW

THE ARGUMENT FOR SENSATIONISM AS DRAWN FROM DR. BERKELEY

BY EDWIN B. HOLT
Princeton University

The old and familiar doctrine that sensations (or, in the newer idiom, *sensa*) are the ultimate units from which the contents of the mind are by some sort of active process built up, was made by Berkeley the basis for his argument that since the mind 'knows' *only* its own sensations, it can never know any outer objects or have, indeed, any reason for supposing that its sensations in any way represent or proceed from such objects. It can hardly be denied, I believe, that on this argument and no other every form of philosophical subjectivism ultimately rests. Let us call Berkeley's argument the argument for sensationism.

In Berkeley's own writings this argument is embedded in a matrix of mutually conflicting propositions and of arbitrary and paradoxical assertions which are of little interest to philosophers to-day, and which obscure the exact formulation of the argument. Probably because he realized this difficulty, Hume abstracted Berkeley's argument from the nonsense with which it was interwoven, and restated it in a more nearly intelligible form; calling it the argument for 'excessive scepticism' as 'drawn from Dr. Berkeley.' It seems to me fairer to this argument to base a criticism of it on Hume's restatement¹ than on Berkeley's original sentences.

¹ Hume's account of it is to be found in his *Treatise of human nature*, Part IV, Section II; and in his *Inquiry concerning the human understanding*, Section XII, Part I, with some counter considerations in Parts II and III. I take Hume's exposition of the

The blind and instinctive view which 'all men' take of external objects, says Hume (9, 173), "is soon destroyed by the slightest philosophy, which teaches us that nothing can ever be present to the mind but an image or perception, and that the senses are only the inlets through which these images are conveyed, without being able to produce any immediate intercourse between the mind and the object. . . . These are the obvious dictates of reason; and no man who reflects ever doubted that the existences which we consider, when we say, *this house* and *that tree*, are nothing but perceptions in the mind, and fleeting copies or representations of other existences, which remain uniform and independent."

Hume says further (pp. 174-5), "It is a question of fact, whether the perceptions of the senses be produced by external objects resembling them: how shall this question be determined? By experience, surely, as all other questions of a like nature. But here experience is, and must be entirely silent. The mind has never anything present to it but the perceptions, and cannot possibly reach any experience of their connection with objects. The supposition of such a connection is, therefore, without any foundation in reasoning."

The above paragraph is the real crux of the argument for sensationism; and it could scarcely be more cogently stated. Hume goes on to show (pp. 175-6) that 'the supposed primary qualities of extension and solidity' are every whit as subjective as are the so-called secondary or 'sensible qualities of objects, such as hard, soft, hot, cold, white, black, etc.'; and finally concludes (p. 177): "Bereave matter of all its intelligible qualities, both primary and secondary, you in a manner annihilate it, and leave only a certain unknown, inexplicable *something*, as the cause of our perceptions; a notion so imperfect, that no sceptic will think it worth while to contend against it."

argument for sensationism from the Inquiry rather than from the Treatise, because in the 'Advertisement' at the front of his Inquiry Hume refers to the Treatise as 'that juvenile work which the Author never acknowledged,' and desires that henceforth the Inquiry 'may alone be regarded as containing his philosophical sentiments and principles.'

Berkeley had, of course, inveighed mightily against precisely such an 'unknown, inexplicable *something*,' 'matter,' which, he said (Principles, VII, XVII, LXVIII, LXXIII, LXXIV; Dialogues, I. 229-31, II. 256, III. 274, 279) was merely an 'unknown quiddity with an absolute existence,' a mere invention to serve as a pretended reality behind, and as a 'support for,' the 'accidents.' But Berkeley was at no point more disingenuous. His 'mind of God' was eminently 'an unknown quiddity with an absolute existence.' And every word which he urges against matter as the supporting reality behind the 'accidents' (sensations or ideas) applies with the same force against his 'mind of God,' which was merely the same supporting reality behind sensations and ideas (Pr. LXXXIX, XC, XCI, CXXXV; D. II. 245, III. 270). "I turn matter into the mind of God," Philonous might have said.

The sentence from Hume, quoted just above, is of importance. Hume conceded an 'unknown, inexplicable *something*' as an irreducible residue *after* the argument for sensationism had transformed both primary and secondary qualities into 'sensations.' Berkeley had found it, and from a purely private motive was determined to dub it 'the mind of God.' Descartes had found it (in the First and Fifth Meditations) in connection with the 'immutable and eternal' properties of geometrical figures, which 'were not framed by me,' 'nor in any degree dependent on my thought,' nor did I arrive at them 'through the medium of the senses.' E. H. Weber (22), in interpreting the phenomenon of feeling touch at the distant end of a cane, was led to suggest that there may be working in us "a substitute for the understanding, that is, the soul," which "without having an insight into the circumstances, is impelled from some unknown cause to order its sensations in accordance with these circumstances, as if by an intellectual instinct." Thus for Weber, too, there were an 'unknown cause' and external 'circumstances' over and above the mind's own 'sensations.' The existence of this alien and irrational element, '*le divers*' of the late Émile Meyerson, which cannot become 'sensation,' is a most important point.

The argument for sensation could not, I believe, be stated more logically or more incisively than Hume has stated it. In conclusion, Hume says (9, 176): "This argument is drawn from Dr. Berkeley; and indeed most of the writings of that very ingenious author form the best lessons of scepticism which are to be found either among the ancient or modern philosophers, Bayle not excepted. He professes, however, in his title-page (and undoubtedly with great truth) to have composed his book against the sceptics as well as against the atheists and freethinkers. But that all of his arguments, though otherwise intended, are, in reality, merely sceptical, appears from this, *that they admit of no answer, and produce no conviction*. Their only effect is to cause that momentary amazement and irresolution and confusion, which is the result of scepticism."

In Parts II and III of Section XII, which are the concluding passages of the Inquiry, Hume gives his own reasons for *rejecting* the argument for sensationism.² His general drift is shown in the following sentences (p. 182): "We need only ask such a sceptic, *What his meaning is? And what he proposes by all these curious researches?* He is immediately at a loss, and knows not what to answer. . . . And though a PYRRHONIAN [an 'excessive' sceptic] may throw himself or others into a momentary amazement and confusion by his profound reasonings, the first and most trivial event in life will put to flight all his doubts and scruples, and leave him the same, in every point of action and speculation, with the philosophers of every other sect, or with those who never concerned themselves in any philosophical researches. When he awakes from his dream, he will be the first to join in the laugh against himself, and to confess, that all his objections are mere amusement, and can have no other tendency than to show the whimsical condition of mankind, . . ." (8, 505). "This sceptical doubt, both with respect to reason and the senses, is a malady, which can never be radically cur'd, Carelessness and inattention alone can afford us any remedy. For this reason I rely entirely upon them; and take it for granted, whatever may be

² All three Parts of Section XII are well worth reperusing in this present connection.

the reader's opinion at this present moment, that an hour hence he will be persuaded there is both an external and internal world."

According to Hume, the argument for sensationism admits of no answer, and produces no conviction. He does not claim that his own reasons for rejecting the argument are a refutation of it. He argues (9, 183) for a judicial poise and an undogmatic attitude of mind, and bids us become 'sensible of the strange infirmities of human understanding.' Nevertheless, if one looks more carefully one finds a certain undercurrent running through Hume's adverse comments, and from time to time it comes to the surface in explicit form; as, for instance, in Section I of the Inquiry (p. 3): "the abstruse philosophy, being founded on a turn of mind which cannot enter into business and action, vanishes when the philosopher leaves the shade and comes into open day; nor can its principles easily retain any influence over our conduct and behavior." This philosophy somehow vanishes in the face of 'business and action,' 'conduct and behavior.' And it vanishes, of course, because it there encounters something alien, some obstacle which *is not* 'my sensation' or 'my idea.' This is that same something which Descartes found even in dealing with geometrical figures, which is 'not framed by me,' 'nor in any degree dependent on my thought.' The abstruse philosophy can live only in the shade, where it avoids and hopes to evade all dealings with the concrete.

This fact, which Hume abides by, is a practical refutation of sensationism. And Berkeley himself hastened to readmit this alien something, now calling it 'the mind of God,' notwithstanding that in order to do so he had to contradict the main proposition on which his argument for sensationism rested. This essential proposition was never anything but a bland insinuation of untruth for dialectical purposes, and it was plainly in his plan to countervail the proposition by reintroducing the alien in a sanctified guise. The fundamental contradiction in Berkeley is, then: there can be no external reality behind our perceptions and ideas—the mind of

God is the external reality behind our perceptions and ideas. Berkeley's self-refutation leaves nothing to be desired.

The issue for us is as to whether there is or is not some alien reality, something not 'mental,' lying behind the sensations and ideas which we conceive to be the contents of the human mind; that is, lying behind experience. In Hume's words (9, 173-5), the slightest philosophy (*i.e.* reflection) "teaches us that nothing can ever be present to the mind but an image or perception, and that the senses are only the inlets through which these images are conveyed, without being able to produce any immediate intercourse between the mind and the object. The table which we see, seems to diminish, as we remove further from it: but the real table, which exists independent of us, suffers no alteration: it was therefore nothing but its image which was present to the mind. . . . The mind has never any thing present to it but the perceptions, and cannot possibly reach any experience of their connection with objects. The supposition of such a connection is, therefore, without any foundation in reasoning."

Firstly, as to the *form* of this argument: Can it be that generations of philosophers have imagined this to be a *reductio ad absurdum*? Yonder table sends me certain sensations through my sensory inlets. And, yes, these sensations have arrived. Therefore, the slightest philosophy shows me that there is no table out yonder, and there are no sensory inlets.

A friend in Persia promised to send me some rugs by steamer. The rugs have just arrived. Therefore, the slightest philosophy tells me that there is no Persia, no friend, and no steamship. Here are the rugs, as evidence.

I would remind the philosopher that if certain premises lead to an acceptable conclusion, the acceptance of this conclusion does not invalidate those premises; and that an *argumentum ad absurdum* is of this other form: If A is true, B must be true; but B is not true, and therefore A cannot be true. The only conclusion, whether formal or material, legitimately to be drawn from Hume's premises is that the *sensations are received*; a conclusion which the philosopher, of course, grants. The pretended conclusion, of sensationism, is an assertion

which rests on nothing, and in fact has no relation to Hume's premises save that it flatly contradicts them. If there is no 'real table,' what business has Hume or anyone else to say that it 'suffers no alteration' as an observer 'removes further from it'; or what business to speak at all of 'the table which we see,' or of 'an image or perception' of it, or of 'intercourse between the mind and the object'? None whatsoever! the absurdity becomes even more acute with the denial of real sensory inlets, for with them is denied the perceiving physical organism entire. Yet this physical organism is premised by Hume's words 'as we remove further from' the table, or by Berkeley's 'at some distance from the mind'; is premised in fact by the very words *sensation*, *image*, and *perception*.³ The pretended conclusion abolishes everything on which the argument rests.

If the supposed conclusion of an argument contradicts its premises, that conclusion was never logically deduced from those premises; at some point the contradiction has been illicitly imported. In the armchair argument⁴ this illicit step is taken in the conclusion itself; in the assertion that a human subject can have, or know, *only* sensations, perceptions, etc.: whereas it was premised that a human subject knows (1) external physical objects, (2) physical sense-organs, brain, etc., (3) his own sensations and perceptions. The argument is formally void. This type of argument enjoys the name *non sequitur*.

I do not too profoundly venerate philosophers, on the whole, but it would be remarkable indeed if they have all been such mere fools as this argument makes them seem to be. And scarcely a philosopher has lived since Berkeley, save only those of the Materialist school, who would not on occasion urge in all seriousness the armchair argument. Perhaps a

³ This paradox is rather accentuated in the presentation given by Karl Pearson in his *Grammar of science*, and in the cut (borrowed from Ernst Mach) which shows the field of visual consciousness framed in by 'sensations of' the nose and eyebrows. The Kilkenny cats were not more preposterous than this cat of Dromore.

⁴ *I.e.* the argument for sensationism. Philosophers since Hume have habitually illustrated this argument by feigning to consider some household convenience, such as a table or armchair.

first clue to this puzzle is to be found in a shrewd observation made some time ago and more recently emphasized by Alfred Sidgwick (18), F. Mauthner (13) and H. M. Kallen (11). In any formal argument, said Sidgwick (18, 75, 81-2, 132, 289), we always as a matter of fact *start* with our 'conclusion,' and then seek to embellish it with some argument or 'proof,' preferably of the impregnable syllogistic type. Our cherished opinion, our 'conclusion,' will appeal more widely, we think, when so tricked out. Beyond question, the philosophers *have* started with their 'conclusion'; even as did Hume, who *begins* the paragraph last quoted: "But this universal and primary opinion of all men is soon destroyed by the slightest philosophy, which teaches us that nothing can ever be present to the mind but an image or perception,". . . Then the armchair argument *follows*, as quoted above. Similarly, the very opening sentence of Berkeley's *Principles* declares that the sole 'objects of human knowledge' are 'ideas.' Unfortunately it is as difficult, psychologically speaking, to detect the weakness of an argument when one eagerly accepts its conclusion, as it is to admit the force of an argument when one finds its conclusion repugnant. And it is certainly not the armchair *non sequitur* that has ever convinced philosophers. There must be some other motive behind the argument for sensationism.

Secondly, there is another equally fatal defect in the argument for sensationism, and one which equally lies within the philosopher's own 'shade,' that is, which develops from the philosopher's own premises. The armchair argument purports to analyse the supposedly physical phenomena (objects, things) into mental or subjective elements; and with no physical or objective residue. And these elements are called 'sensations.' It premises, as we have already seen, a person considering the objects about him, perhaps his armchair, in a 'common sense' or naïvely realistic attitude. This is the attitude, indeed, which even the most convinced solipsist (writing for the edification and approval of his fellow solipsists) must adopt when he dips his pen in ink and plies it across his paper.

This analysis is invariably hasty and superficial—clearly enough a merely perfunctory gesture of the philosopher. There is much diversity of opinions as to just what aspects of the armchair or other object are ‘sensations.’ But all of the secondary (sensory) qualities are allowed to be sensations *κατ’ ἐξοχήν*. Spatial aspects and relations are sometimes (not always) alleged to be ‘represented’ by ‘kinæsthetic sensations.’ Temporal relations and the experience of phenomenal motion are ignored. But on two points there is unanimity of opinion: the ‘sensations’ are *discrete* entities, and so far as delivered by the external object (for this is premised) to the ‘mind,’ they are in chaotic *disorder*; a ‘kaleidoscope of sensations,’ as it has been called by N. Kostyleff (12). That is, this argument solemnly affirms that a plain man’s experience of his armchair consists of a great number of particles (‘sensations’) in the signally disordered condition of a swarm of flies. Lest this be disputed, let us consult the philosophers’ *ipsissima verba*. Berkeley, in his *Principles* and the *Three Dialogues*, refers habitually to a phenomenal object as being ‘only so many mere sensations’; and he employs the following metaphors: ‘combination’ of sensations, 5 times by actual count; ‘collection,’ 3 times; ‘blending,’ 3 times; ‘mixture,’ twice; ‘congeries,’ ‘uniting together,’ ‘concretion,’ and ‘assemblage,’ at least once each. (And I have doubtless missed some instances).

Hume writes, in the *Treatise* (8, 495), “that what we call a *mind*, is nothing but a heap or collection of different perceptions, united together by certain relations”; and again on the same page, ‘a connected heap of perceptions.’ If ‘nothing but a heap or collection,’ the perceptions are *not* connected by any significant relations; or indeed by any relation except *together with, plus, and*, or something of the sort. Dr. F. C. S. Schiller (17, 275–6, 282) has commented on this disconnectedness of the ‘impressions’ according to Hume and his later critics.

As if to leave no possible doubt on this point, *Vaihinger* (21, 286) is even more explicit: “Out of the chaos of sensations develops distinct and individuated cognition; in that chaos

there is as yet no perception of any particular *thing*, for the great vague nebula of sensations first gradually takes on a swirling motion and the single cohering particles lump themselves gradually into perceptions of things, to the cognition of particulars." And this is said *à propos* of sensationism, not, as might be surmised, *à propos* of some mythical condition of the 'foetal mind.' Kant's attitude on this point is well known. In Rougier's words (16, 401): "That which characterizes the elements given to the sensibility, according to the author of the Critique, is their state of complete dispersion. It is the categories of the understanding which unify in *a priori* syntheses the alien given of intuition," etc.

Such is the philosopher's sober analysis of a phenomenal object; an analysis which has set the problem of philosophy since the time of Berkeley and Hume. It is as if one were to chop a physical table into a heap of splinters, and were then to allege that there had never been a table there.

This analysis deliberately neglects to enumerate precisely those 'organizing relations' to which philosophers, on all other occasions, are so peculiarly sensitive. Whatever the elements of the phenomenal armchair or table, those elements are *phenomenally* organized; they are *not* a 'mere' heap, flux, collection, congeries, or kaleidoscope, nor yet a vague chaotic nebula in swirling motion. Considered purely 'phenomenally,' the elements composing the armchair, whether 'sensations' or whatever else, present a stable organization in phenomenal space and in phenomenal time. If there is any phenomenal motion, it is the motion of groups of more or less stably organized elements in phenomenal space and time. It is not the flux or swirling motion of a chaotic nebula.

And why has phenomenal motion been passed over in silence: why has a most nearly static object, such as an armchair, been regularly selected as the phenomenon to be analyzed; when the argument is offered as a basic analysis of *all* phenomenal experience? Would it be as plausible to aver that a tight-rope performer, an eclipse of the sun, the unfurling of a night-blooming cereus blossom is phenomenally a mere heap, collection or chaos of sensations! There is no ground,

unless some unvoiced and hidden prejudice, for alleging that spatial and temporal relations and the experience of motion and change are in any way less 'phenomenal' than the (always substantively conceived!) 'sensations.' And yet these experiences of relation and change are illicitly set aside, for the purposes of this argument, by a procedure which apparently ensnares the reader through its very audacity.

In connection with the crude *non sequitur* contained in the argument for sensationism I remarked that 'some other motive' must have blinded philosophers to this lapse in logic. Similarly here, the singularly desultory 'analysis' presented in the armchair argument leads us to suspect that the same motive has again decided the philosopher's 'conclusion' in advance, and that the argument, the 'analysis,' which purports to lead up to that conclusion is in fact merely, once more, an embellishment and an after-thought. It will be worth while, I believe, to seek to uncover that hidden motive which, in other cases than that of Berkeley, has determined in advance the sensationist 'conclusion' and so, indirectly, the meretricious and unworthy 'argument.'

When a philosopher sits down in his armchair and works at his table, his *actions* prove that he is under no delusion about these phenomenal objects being a mere swirling nebula of sensations. His actions definitely refute his words. But the average philosopher does not indulge in action overmuch. As compared with men of action, the typical man of reflection does little more than rise up and sit down and ply his pen. It is not for the philosopher to walk a tight-rope, nor to build a flying-machine; not for him to watch with ardent curiosity the slow unfolding of a cactus flower; nor indeed can most philosophers successfully drive a nail. Has this some bearing on the nature of his reflective processes?

Hume himself has given us the important clue (8, 505): "This sceptical doubt, both with respect to reason and the senses, is a malady, As the sceptical doubt arises naturally from a profound and intense reflection on those subjects, it always encreases, the farther we carry our reflections, whether in opposition or conformity to it" (9, 182). "When

he [the sceptic] awakes from his dream, he will be the first to join in the laugh against himself, and to confess, that all his objections are mere amusement, and can have no other tendency than to show the whimsical condition of mankind,"

'The first and most trivial event in life,' even the act of sitting down in an armchair, does indeed refute the entire philosophy of sensationism. Philosophers such as Galileo or Newton addressed themselves to the study of reality, they had direct *contact* with it, and their philosophy gives evidence of this contact. This was not the case with the philosophers of the subjectivist tradition, Berkeley and his successors. Their biographies and their works sufficiently show that they lived ecclesiastically or academically cloistered lives, that they were shy of reality, and ignorant of it save for that second-hand knowledge which can be imparted by mere words. In short, they were introverts. The preposterous Hegel was the supreme example. Hume and Schopenhauer, perhaps the furthest removed from Hegel, almost alone showed a competence in observing and thinking about reality. These subjectivists, save the two just named, would never have been called by others 'great philosophers' were it not that western culture has itself come to be preëminently verbal and introverted,⁵ so that there has been a large and eager public to accept blindly the subjectivist Gospel of Escape.

Seen from this point of view, the 'great Kant' was no exception to the others; and as he essayed to write the *Prolegomena* to any future metaphysic, so there may be, all unsuspected, a prolegomenon to any future Kant. His early interest in astronomy is often cited to show that he was an empirical scientist, even as Newton. Whether he attained proficiency here or not, the contact with reality which astronomy affords is so exceedingly remote that an astronomer is, notoriously, hardly to be distinguished from a 'pure' mathematician. And even in astronomy Kant's interest was not sustained. In his life and works he evinced a distaste for anything concrete, anything *anschaulich*. He was persuaded,

⁵ Cf. Mauthner, 14, vol. I, 64-6.

it becomes obvious, that thus he 'transcended' the temporal and the accidental, and dwelt on high, *hochgebildet, vornehm, göttlich sogar*, among the most rare and elegant abstractions. A sober empirical psychology knows that such disdain is merely a compensatory rationalization which covers a mental condition of ignorance and fear of reality, and that these were produced by unfortunate incidents in early childhood. We shall have to return, presently, to this aspect of the introverted mind.

Hume's shrewd remark about 'the most trivial event of life' puts us in a position to understand *psychologically* the argument for sensationism and its vogue. We have seen that the denial of external reality on the ground that we do have images, perceptions, etc., is, formally or logically considered, the most bare-faced *non sequitur*; and will not have convinced anybody.⁶ I remarked in that connection that 'some other motive' must account for the widespread acceptance of the argument. That other motive we now see, with Hume's help, to be a temperamental aversion to reality, an aversion profound enough to produce a practical denial, a more or less general mental blindness to it. Many persons are so blinded, and they applaud any pretended argument that encourages their temperamental failing. This is called emotional or wish thinking. But did we ourselves detect that the first sentence quoted from Hume at the head of this article contained *both* the emotional foregone conclusion and, immediately following, a contradictory proposition advanced in support of it? So much for 'the strange infirmities of human understanding' and 'the whimsical condition of mankind.'

Considered formally and quite within the philosopher's own deepest 'shade,' the argument for sensationism is not an argument. And even Hume's genius could not turn it into one. It is a mere *non sequitur*.

⁶ I acknowledge that Hume's statement that it 'admits of no answer' puzzles me, unless Hume meant 'because it is no argument.' But the context shows, I think, that Hume did not so mean. Perhaps Hume himself liked sometimes to go into 'the shade' with the philosopher and play truant to reality. That would be tantamount to embracing, for the time being, the Gospel of Escape.

In the same way, the pretended analysis of a phenomenal object into a disorganized heap or flux of discrete sensations is presented and is accepted seriously; the philosopher's aversion to reality preventing him from looking once again at the armchair and realizing that his 'analysis' was a ridiculous travesty. I doubt indeed whether as a fact any philosopher, in composing this nonsense, ever actually looked at an armchair. "Pooh, what is an armchair: I've seen that sort of thing often enough." There is a hint of disingenuousness about this analysis. A philosopher is not blind to organizing relations, and he knows perfectly well that he will 'find' and restore these overlooked organizing relations exactly when it suits his purpose to do so: they will reappear, transfigured, as the autonomous creation of a mythical faculty called the Understanding. Berkeley played the same trick, when he denied a supporting reality behind the accidents and then restored that very same supporting reality under the designation 'mind of God.'

Each of the two objections so far considered is, by itself, a sufficient refutation of the argument for sensationism. And each is taken on grounds that are quite within the philosopher's own premises. Should any reader still feel a lingering attachment to this doctrine, I would beg him to read the late Prof. Melchior Palágyi's (15, 1-24) brilliant and profoundly original refutation of it. Palágyi, a mathematician and philosopher, was one of the few thinkers who have realized the epistemological significance of the doctrine of sensationism and the importance of refuting it. He attacks it on the (perfectly valid) grounds that sensations (*i.e.* sensory qualities) are not simple and are not elements of consciousness which the mind receives merely passively. His argument rapidly develops into regions of which few of us have even suspected the existence. And presently sensationism with its epistemological consequences is left far behind, like a battered toy on the play-room floor.

Now it is not true that the mind receives 'sensations' through 'sensory inlets,' as a vast number of phenomena familiar to psychologists sufficiently attest. This empirical

evidence cannot be presented here; but it may be in place to examine here the mental process which is responsible for the notion that discrete 'sensations' are the elements or atoms of the mind.

F. Mauthner, in his *Kritik der Sprache* (14), has insisted that all thinking is metaphorical. And he distinguishes carefully between thinking (or what we should perhaps call discursive thinking) and empirical observation. I do not know how far this opinion is generally accepted; but it is strongly confirmed by most competent linguists, by the philosophy of As-If, and by the brilliant studies of F. Mauthner and É. Meyerson. At any rate, metaphorical or analogical thinking has long been a well recognized type of thought. According to the motor re-creation theory of the mind, metaphorical thinking must be well-nigh incessant.

Objects in stimulating sense-organs produce no consciousness, but they produce movements of various motile members, which movements are, in general, adient to the stimulating objects. And such movements point to the objects, and explore, *i.e.* re-create, their contours and parts. And what the organism so *does* is its consciousness, its mind.

The organism, when awake, is thus perpetually responding with its motile members, specially its optical apparatus, to features of its environment; some of these response attitudes are sustained and more or less chronic, as I have elsewhere (7) explained in connection with 'integration'; others are fleeting little explorations, directed now to one now to another feature of the environment, as passing events, by stimulating the organism, dictate. This frequent shift in the direction of response is called the 'movement of attention.'

The proprioceptive sense-organs with which muscles are provided ensure that any sequence of movements which is repeated a few times becomes more or less consolidated and fixed as a 'chain reflex.' If once started, such a chain of movements tends to be continued by the proprioceptive mechanism, and to play itself through like a phonograph record. So it comes about that a child gradually acquires a repertoire of set little responses which it has learned by

frequently exploring (re-creating) the objects which it most often encounters: such as some parts of its own body, its clothes, its favourite toys, the common implements of its daily life, and any features of the domestic scene with which it has come to be concerned, or 'interested'; this latter meaning, 'which it has often encountered as an *obstacle*.' Such objects, as stimuli, touch off more or less well organized chain reflexes, the habituated exploratory responses specific to the objects themselves. It is because we have often encountered circular objects as obstacles, for instance, that we can trace circles in the air or on paper when stimulated to do so merely by words; and similarly for other spatial contours. The child 'recognizes' such familiar objects readily, manipulates them with facility, and employs them with such 'intelligence' as it has so far acquired.

But a penalty is incurred by this very facility. The response to a familiar and also present object is under *two* guiding influences: it is an exploratory re-creation guided by the object itself, and at the same time it is a chain reflex more or less rigidly fixed in the proprioceptive mechanism. And the world of real objects will produce incessant conflict between these two guides, between the world as we already know it, and the world as we have yet to learn it. If a little girl who loves her teddy-bear is given a live kitten, the two furry quadrupeds are enough alike as stimuli, to excite in the child her habitual reactions. She treats the kitten *as if* it were a teddy-bear, inanimate. And amusing scenes are always enacted when the little girl tucks the kitten more or less forcibly into the teddy-bear cradle 'because it is time to go to sleep.'⁷

Whenever some unfamiliar object or configuration as stimulus is similar enough to an object with which we are already familiar, to *start* those chain reflexes which the familiar object habitually excites, the chain reflexes will continue of their own motion; unless some interfering feature of the unfamiliar object also excites other response motions which

⁷ The results for the child are obstacles, motor block, trial-and-error learning, and eventual discrimination.

antagonize those first excited. The new object will be perceived *as* the old and familiar one; will be so comprehended, or 'apperceived'; and will be so treated practically: *until* the new object presents some new and unexpected obstacle. Since every man and every higher animal is a veritable magazine of countless such chain reflexes, these will inevitably be touched off by sensory cues to which they are in many cases inappropriate. Those prehistoric adventurers who first caught sight of hippopotamuses swimming about, perhaps in some African river, were already well acquainted with that animal of early domestication, the horse. The name given to the strange beasts proves that the discoverers reacted more or less as follows: Why, see those strange horses swimming, over there. No, they are not quite like horses. They must be a new kind of horse. It is clear enough: these are *river-horses*. Eureka.⁸ And does the modern taxonomist conduct his thoughts otherwise? Such is the mechanism of metaphor; and it is the mechanism of that which psychologists call 'apperception,' or 'apperceptive mass.'

Wherever the novelty to be experienced is not a tangible object but a configuration of many stimuli, a sad medley of the wonted responses is often set in action, and the metaphors which emerge from the turmoil tend to be haphazard and inept. After a sermon or lecture, if one files out with the throng, one overhears such comments as these: "What lofty thoughts! I felt that I was being borne up into the very empyrean;" "What a deep thinker! Never have I listened to utterances more profound." And the other extreme of appreciation shows the same bewilderment: "What airy nonsense! He went up and up, like a gas-balloon;" "An hour and a half wallowing in the depths. Really, bathos could not lower go." The speaker is happy or he is unhappy; in either case he has been plentifully stimulated. And the experience resolves and clarifies itself for him in metaphor; in terms of that never dormant antithesis—up and down.

⁸ In a similar way the elephant was once identified with the cow. "Luca bos. Lucanian cow, for elephant (because the Romans first saw this animal in Lucania, in the army of Pyrrhus)." (fr. Harpers' Latin Dictionary)

But which is up and which is down is a matter of personal idiosyncrasy. Some of us are happy when we soar, and others of us when we delve; as incidents in our childhood determine. It is one aspect of man's whimsical condition that he chatters, interminably chatters nonsense. And none of us escapes. I make no doubt that these present pages could be shown to teem with just such metaphorical fumbling.

In observing concrete objects and processes we undertake to re-create the contours and parts and to follow their movements with our motile members. Such following can never re-create the reality exhaustively, never more than very roughly approximate it: firstly, because we can follow but one contour or movement at a time, nor that very minutely; secondly, because during that following, stored response systems (concatenations) are inevitably set in motion and at some point gain the upper hand.⁹ And then we cease to observe (*i.e.* re-create under guidance), and both 'perceive' and act upon the object of our cognition *as if* it were some (more or less similar) object with which we are already familiar. At some point the metaphorical or, as it may properly be called, the 'subjective' element creeps in. The metaphor may be a tolerably apt analogy, or it may not.

Those unfortunate children who have been made fearful and shy of reality, introverted, are at the same time made unobservant. Their repertoire of stored response systems, which *eo ipso* is their repertoire of stored 'ideas,' is meager. The result is twice unfortunate. On the one hand they shirk direct observation and fall back on metaphor, that is, think in terms of the old and familiar, all the more readily; and on the other hand, this metaphor, since it can come only from the meager supply of stored experiences, is foredoomed to be inept and childish. Such introverts, inevitably, delight in what is called 'abstract' thinking. And I believe that works, or passages in works, which incline us to say to ourselves: "How remarkably abstract this is," will generally be found to consist of a ubiquitous play of rather puerile metaphor.

⁹ And because of other limitations. The difficulties here encountered are of course far-reaching. Our instrument of cognition is frail.

The three-step of thesis, antithesis, and synthesis was Hegel's obsessive metaphor. And it is puerile because not the texture of reality suggests it, but Hegel's chronic chain reflex. He could think *only* in three-step. E. R. Jaensch (10, 354, 359) and others have well remarked that, as an indispensable preliminary to competent 'abstract' thinking, any person must '*saturate*' himself with a great variety of concrete experiences. And he should begin the process in his early childhood. An introvert scarcely begins.

After this preamble, we shall understand readily how three very casual metaphors (analogies) have shaped the theories of sensation and sensationism.¹⁰

The first of these metaphors is that the 'sensations' are conceived materialistically, as physical bodies, not very large and not very heavy, much like the physicist's atom. They are the lineal descendants of those finest, lightest, and most mobile particles that entered into the *voûs* of Anaxagoras. They are, in the theory, effectually reified, thing-ized; and thereafter merely to dub them 'mental' (atoms) in no wise purges them of their materiality. This fact has been pointed out by many writers, and is patent in any discussion of sensationism; as in the cases previously quoted, where we read of 'heaps,' 'collections,' 'mixtures,' 'concretions,' 'assemblages,' etc., etc. of 'sensations.' Rougier (16, 406) remarks: "Sensations are not such discrete atoms, unrelated to one another, as the English associationists imagine them to be." Schiller (17, 276) speaks of "the atomized 'matter of sensation'" on which the understanding is supposed to impose its categories. Palágyi (15, 48, 106) charges Kant with the same pictorial thinking "when he describes sensations as the stuff [*Stoff*] and space and time as the form of sensuous intuition." E. R. Jaensch, who leans strongly toward Kant, speaks (10, 309) of "the Kantian distinction between the 'form' and the 'matter'" of our intuition. W. Stern seems to have believed at one time (19, 27) that, "As a matter of fact the spatial and temporal intuitions have no other function [*Aufgabe*] than to bring order into the chaos of our perceptual

¹⁰ Cf. in this connection A. Binet, 2, 611-618.

world." I cannot help querying, further, to what extent the disciples of Kant figure to themselves, implicitly or explicitly, the 'categories' as screens or sieves placed between themselves and the *Dinge an sich*, and effecting a physical selection as to the size and shape of the 'grains' (cf. Jaensch, 10, 13) which can come through.¹¹

The second metaphor which is involved in the doctrine of sensation as well as in that of pansensationism, is the notion of physical sampling. The 'sensations' are minute samples of the world outside, which come to us through the sensory 'inlets'; they are, in short, the ancient 'visible species,' etc. That this is explicitly denied in the *conclusion* of the sensationist argument has nothing to do with its being essentially involved in the premises. Descartes' story of the soul receiving 'sensations' in the pineal gland would lack cogency if these sensations were *not* thought of as representative samples. That connotation is an essential part of the meaning of the word 'sensation,' and of the word 'sensuous' (intuition). The reader can test this for himself if he will substitute the word 'entity' for 'sensation,' in the next sensationist argument that he chances upon.

The third metaphor serves to make comprehensible how the understanding, the mind, or the soul can construct its own subjective world out of the 'sensations.' These latter are little building-blocks, and the understanding puts them together according to the 'laws' of space and time. The analogy varies somewhat with the individual. Thus, Stern (20, 73-4): "The main theses of these widespread and variously shaded theories [sensationisms] are: the passive reception of sensory impressions coming from without; the original atomization of the content of consciousness into mere little mosaic-stones ('sensations'), the building up of the psychic life by mechanical consolidation of these originally

¹¹ In his discussion of 'externality' vs. 'internality' of relations F. H. Bradley (3, B'k. I, Chap's II and III) has reified the idea of *relation*, so that it is equivalent to a substantively conceived term. He then argues that this 'relation' between two terms does not relate, but merely separates, them. From this he concludes that the relation between two terms must be 'internal' to the terms related. I cannot see that his argument is more than a nonsensical playing with irrelevant metaphors.

segregated elements. Now none of these theses," Stern continues, "tallies with the actual development of experience in the child;" . . .

No objection would be raised if sensationists employed metaphors, scrupulously, in order to give a clearer exposition of their doctrine. But, as also Binet (2, 612-618) has pointed out, the argument for sensationism is *nothing but* a bouquet of metaphors. The metaphors *are* the argument: behind the metaphors is—nothing. For the sensationist, instead of observing and studying attentively the phenomenon of cognition, has merely glanced and *seen* it *as if* it were ethereal effusions streaming off of physical objects, to and into some other fellow's 'sensory inlets,' etc.; exactly as our early adventurers looked at hippopotamuses and *saw* horses, saw only the familiar and expected (a psychic feat which the motor theory can explain). There are facts indeed there where the sensationist pretended to look, but he saw them not: he saw only that which, from among his scanty store of past (concatenated) experiences, his introvert eye expected to see.¹² Nothing did he learn by looking, and so nothing lies behind the childish metaphors of his argument.

We have seen that a preconceived conclusion has blinded the sensationist, and *eo ipso* the subjective idealist, to the strange *non sequitur* of his argument, and that the same preconception has blinded him to the shortcomings of his pretended analysis of a phenomenal object. This preconception, we have also seen, is not primarily determined by the 'reason,' but derives from a deep-seated physical aversion to the world and its affairs; this aversion deriving in turn from harsh experiences, or perhaps a fragile nervous constitution, suffered in childhood. In the preceding paragraph we saw one of the ways in which such an acquired aversion (physiological, postural) can influence and indeed govern what are called the 'dictates of reason' (*cf.* Binet, 2, 600). The introvert and the recluse hates to look at reality; and can by no means bear to look at it long and searchingly. Such a diagnosis is borne

¹² Seeing, as well as thinking, metaphorically is a species of normal and every-day hallucination.

out by yet a further consideration. The critical examination of a phenomenal object and the discovery that it is 'composed of' sensations, atoms, or whatever else, does not abolish or in any wise change that object. If the armchair is composed of sensations, it is none the less the same armchair with which I started; and even a sensation-armchair is not my personal Faculty of Understanding. My examination does not 'reduce' the armchair to a promiscuous congeries of sensations; does not reduce or dissolve it in any way. If I undertake to teach a dog to sit up and beg for candy, the philosopher's opinion, whether right or wrong, that the dog is made of 'sensations' (or quiddle-quoddles) is utterly irrelevant to the transaction; or to any other transaction which I may attempt in this phenomenal world. What has the philosopher altered or accomplished by his 'discovery' that all is 'sensation'? Why does he rejoice?

The literature of sensationism and of subjective idealism affords a plain answer, which may be formulated thus: Every least part of this crude and despicable physical world is *fundamentally* only sensation; therefore I am free to ignore it. I shall *possess its secret* if I ponder deeply the fundamentals:—sensation, intuition, time and space, the Understanding, and the transcendentality of the Pure Reason. Now there is no vestige of truth in this formula; as the present paper has undertaken to show. The subjectivist's deep aversion (postural) impels him to turn his back on the physical world (as far as this is possible); and his verbal formula relegates that world to the same remote status in his *verbal* universe of discourse; which is the subjectivist's chosen plane of activity. His formula is an *incantation*, in the strict sense. It conjures away the physical world; and it is, whether spoken aloud or whispered softly, the cardinal motto of the Gospel of Escape. The verbal formula is subordinate to the instinctive action, and the instinctive action is produced by the body's neuro-muscular connections (synaptic). The contrary notion, a legacy of philosophical rationalism, that action is submissive to the word is a distortion which has gravely confused both philosophy and psychology. Rejecting it, we

shall begin to see the connection between nerves and muscles and the topping flights of 'transcendental' reason.

Religion is the opiate of the masses, and subjectivism is the opiate of the intelligentsia; and George Berkeley is a natural link between these two Gospels of Escape.

We now have, I believe, both the refutation and the explanation of the argument for sensationism. Hume was right: "this sceptical doubt . . . is a malady." Yet the whole fabric of subjectivism is implicit in the much older psychological doctrine of 'sensation.' And the notion that "the contents of the mind are either sensations or ideas" (of which Locke and other predecessors of Berkeley are not to be absolved) effectually precludes *any* solution of the problem of knowledge; that problem being: How can the mind *know* that which is *not mental*? It is metaphorical thinking, again, that leads to the *impasse*. The sensations and ideas, so many 'monads,' are conceived more or less like beans in a firkin (*cf.* Binet, 2, 613); and how shall either beans or firkin 'know' the other things in the store-room? This metaphor must be dismissed, and the concrete process of knowing must be more attentively studied.

In the early paragraphs of this paper I pointed out that Descartes and Hume found an 'unknown, inexplicable *something*' as an irreducible residue *after* the argument for sensationism had transformed both primary and secondary qualities into 'sensations,' a something 'not framed by me,' as Descartes said, 'nor in any degree dependent on my thought.'¹³ Berkeley's 'mind of God,' so far as the words have any meaning, would seem to be the same unknown, inexplicable something. And, notwithstanding the tendency of sectarian philosophers to magnify hair-splitting distinctions of no importance whatsoever into colossal contrasts, I shall venture to affirm that wherever Kant was led to acknowledge a *Ding an sich* he had in mind the same elusive reality lurking somewhere on the other side of the sensory data. But Kant's attitude in regard to the *Dinge an sich* was vacillating and ambiguous, so that even his faithful followers have felt free

¹³ And *not* arrived at 'through the medium of senses.'

to accept or to reject his trans-sensational realities. For the most part they have rejected; as, for instance, Windelband, who unhesitatingly declared (23, 256) that "the hypothesis of *Dinge an sich* is a fiction." So unanimous has been the swing in this direction (cf. Erdmann, 4) that it seems to have been forgotten that any one ever did or ever could take the things-in-themselves seriously. Eduard von Hartmann (5) is one philosopher who did precisely that, however, and his conclusions are of especial interest to anyone who sees fit to reject *in toto* Berkeley's argument for sensationism.

In a careful and often line by line criticism of the relevant passages in the first Critique, Hartmann shows, as he believed, that Kant was quite unjustified in banishing the things-in-themselves *so completely* from the field of direct cognition. And he concludes (5, 34): "The outcome of this examination is, that the theory [Kant's] which was going to *explain* how experience is *possible*, has *demonstrated* that experience is *impossible*, since it has shown that all that seems to be experience is absolute illusion." Perhaps the Neo-Kantian temperament does not repudiate this conclusion. At any rate Windelband (23, 266) seemed to be eminently satisfied with his own interpretation of Kant according to which there is no *Ding an sich*; there are only empty space and the phantasy; empty space "in which therefore the world-principle can be only—phantasy." And 'phantasy' is what Hartmann calls 'illusion.' Hartmann then proceeds to defend against Kant himself the *Dinge an sich* and their status as objects of actual, though partial and imperfect, cognition. They are by no means utterly unknowable. On these lines Hartmann eventually developed his 'transcendental realism.'

The issue comes down to this:—either the Berkeleyan argument for sensationism is valid, and there are no things-in-themselves; or the argument for sensationism is a silly *non sequitur*, and there are things-in-themselves which can be in a measure truly, but never completely, known. The problem of cognition, then, would cease to be a farce. And the net outcome of the Kantian movement would be what we might perhaps call the 'discovery of the mind': the discovery,

that is, of several previously unsuspected distortions and aberrations ('categories') to which human cognition is *essentially* subject. Our human instrument of cognition is frail, and introduces not a few characteristic and serious errors in the process. If ever philosophers will consent to reject the argument for sensationism and psychologists to reject their doctrine of sensations, then indeed I believe that they will find themselves unexpectedly coming together and working harmoniously on a common task—the *more complete* discovery of the mind.

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STUTTERING AND THE CONCEPT OF HANDEDNESS

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Various writers, particularly Dunlap (3) and Buford Johnson (10), have discussed certain aspects of the issue as to whether there is any relation between handedness and stuttering. Their statements have precipitated the necessity for a clarification of the issue itself as well as for the regimentation of research data pertinent to the issue.

In order to orient ourselves for this particular discussion of a rather restricted phase of the field of speech pathology, we refer advisedly to the very fundamental fact that an obvious relationship exists, neurologically, between handedness and the speech function. This fact has been most clearly demonstrated in connection with certain studies of aphasia. Probably the most comprehensive and certainly one of the most scientifically rigorous investigations of aphasia yet reported is that of Weisenburg (34). The following statements are quoted from his report:

"On the preliminary question of localization in the right or left hemisphere, the study presents material from both aphasic and control groups.¹ Analysis of the 98 cases of unilateral cerebral lesion shows:

"1. Aphasia or a history of speech difficulty at the time of the attack in 65 of the 67 right-handed patients, with demonstrated lesions of the left hemisphere or neurologic signs of such lesions.

"2. Aphasia in 2 left-handed patients, 1 with a demonstrated lesion in the right hemisphere and 1 with a demonstrated lesion in the left.

"3. No aphasia or history of it in 22 of the 23 right-handed

¹ The control groups with regard to this aspect of Weisenburg's study consisted of patients with cerebral lesions but without aphasia.

patients with demonstrated lesions of the right hemisphere or neurologic signs of such lesions.

"4. Aphasia in 3 of 5 ambidextrous patients with a lesion in the left hemisphere, and in the 1 ambidextrous patient with neurologic signs of such lesions in the right hemisphere.

"Thus among the 92 clearly right-handed or clearly left-handed patients there are only 4 exceptions to the rule that aphasia appears with lesions of the dominant hemisphere as indicated by handedness, and fails to appear with lesions of the non-dominant hemisphere. In other words, the dominance indicated by handedness is a criterion of the crucial hemisphere for speech in about 95 per cent of the cases."²

With these basic facts in mind, the student of speech pathology, faced with the profound neurological phenomena involved in stuttering, may well undertake a thorough investigation of any demonstrable relationships between stuttering and handedness.

In a narrow sense handedness refers to the relative skill, strength and usage of the two hands. In this sense a right-handed person is one who uses the right hand significantly more than the left hand, and whose right hand is more skillful and stronger than the left.

Closely related to this meaning of handedness is the concept of the 'preferred hand,' a concept that is still more restricted in meaning. On the basis of this definition, a right-handed person is simply one who prefers to use the right hand to a significantly greater degree than the left hand.

Such concepts are characterized by emphasis upon the peripheral manifestations of handedness. Other concepts, however, emphasize the central or neurological aspects of handedness. With emphasis in this latter direction, one distinguishes the peripheral manifestations of handedness from the central neurological concomitants of those manifestations. From this point of view, the concept of handedness

² We suggest, in no dogmatic sense, that a clue to an explanation of the exceptions (the 5 per cent of the cases in which the dominance indicated by handedness did not seem to be a criterion of the crucial hemisphere for speech) may be found in the well-known fact that certain individuals who appear to be right-handed give a history of having been left-handed early in life. The reverse is true, also, in a few cases.

emphasizes the functional relationships among parts of the central nervous system, relationships which have been shown to be associated with the peripheral manifestations of handedness.

In order to clarify our meaning, we may cite a study made by Peterson (23).³ Peterson ascertained the hand preference of rats in an experimentally controlled food situation. He then subjected the rats to operations in the motor and somesthetic regions of the cerebral cortex contra-lateral to the preferred hand. Following the operation all rats, except the one in which the lesion was least extensive, transferred either abruptly or gradually to the consistent use of the hand contra-lateral to the uninjured cerebral hemisphere. No paralysis was detected in the animals; and face-washing, scratching and walking seemed to be normally performed by all of the animals within twenty-four hours after the operation.

This study illustrates what is meant by the central or neurological aspects of handedness, as distinguished from its peripheral aspects. It demonstrates, moreover, the intimate relationship between manual activities as such (in the rat) and the condition of the regions of the cortex anatomically associated with the two hands. It demonstrates also that alteration of the functional relationship between the two cerebral hemispheres (due to lesion in the motor and somesthetic regions of one hemisphere) affects hand usage or preference.

One must make a distinction not only between the peripheral and central aspects of handedness, but also between handedness and sidedness. Sidedness refers to the total condition of laterality as manifested in handedness, eyedness, footedness, and even in sensory and perceptual functions, as well as in general bodily orientation with regard to laterality. As in the case of handedness, so in the case of the broader concept of sidedness, a distinction is to be made between peripheral and central aspects.

Moreover, peripheral manifestations of sidedness are to be distinguished one from another with reference to the relative degrees to which they have been or can be affected by training,

³ Weisenburg's study referred to above is also pertinent in this connection.

directly or indirectly. This consideration is crucial to the issue as to the relative influences of environmental and of biological factors in the determination of sidedness, considered with regard to either its peripheral or central aspects. It is, furthermore, extremely crucial to the problem of measurement of sidedness. With specific reference to handedness, for example, strength and steadiness, together with speed and precision of movement, as measured by means of strength of grip, tapping rate, and quality of handwriting, are factors which might reasonably be assumed to be affected by training to a relatively high degree. The more they reflect the influence of training, the more they may mask the innate organization of the central nervous system. The point to be stressed is that a test may yield a relatively accurate measurement of some peripheral manifestation of handedness (or sidedness) without yielding an accurate measurement of its central or neurological aspects. In reporting measurements of laterality it is highly essential that reference be made to the probable degree to which the measurements refer to peripheral or to central aspects of laterality.

In view of these statements we shall consider the several issues involved in the present discussion of stuttering and handedness. We shall list these issues before discussing them one by one.

1. Is there any relation between the onset of stuttering and change of peripheral handedness?
2. Are stutterers as a group different from non-stutterers as a group with respect to peripheral sidedness?
3. Are there differences in the hereditary backgrounds of stutterers and non-stutterers with respect to peripheral aspects of sidedness?
4. Are stutterers as a group different from non-stutterers as a group with respect to central neurological aspects of sidedness?

II

Although the first issue is, from many angles, not the most important, it is the most commonly discussed. In regard to it Dunlap and Buford Johnson have made definite statements.

Dunlap (3, p. 198) states the issue and his conclusions regarding it in the following terms: "There has been popular a theory that left-handed children, who are trained to use their right hand properly, are especially liable to acquire stammering. This popular notion was at one time bolstered up by explanations based on an old theory of brain centers, and has been recently made the foundation of one of the numerous 'systems' of curing stammering. So far as studies show, there is no basis for the old theory, and parents of 'left-handed' children may not fear to have the children's right hands trained, provided the training is correctly done. There is no doubt that the use of brutal and incompetent methods of training children, not only in respect to use of their hands, but in any way, may contribute to the production of stammering, or any other neurotic trait."

According to Dunlap, therefore, the issue is to be stated thus: Is the change from left-handedness to right-handedness, in and of itself, a factor in the cause of stammering or stuttering? His conclusion is that it is not a factor unless 'brutal and incompetent methods' are used in making the change. He cites no research data in support of or in opposition to this conclusion.

Buford Johnson (10, p. 181), in discussing the relationship between handedness and stuttering, says: "A further confusion in treatment of left-handed children arises from the belief that stuttering or stammering may ensue. It is claimed that a disturbance of the language centers of the brain is caused by changing the hand activities. If the right-handed child becomes skillful in a left-hand activity, fear is rarely manifested over possible brain disturbances. Critical analysis of data concerning the relative frequency of stammering among children to whom training has been given in changing from left-hand to right-preference in manual acts gives no basis for explanation of stammering as a result of this change. The origin appears to lie in emotional disturbances during the initial stage of speech development. . . . If the method used in attempts to force an early preference of right hand are such as to keep the child under tension and to cause frequent emo-

tional upsets, stammering speech may ensue. Any other factors in the child's environment that produce frequent emotional states of such a degree as to interfere with speech also cause stammering. The point to be emphasized in regard to a change in handedness is the degree of emotional disturbance aroused. . . . Under proper conditions of training in transfer there appears to be no interference with speech."

According to Buford Johnson, therefore, the issue is to be stated in the same way as it is by Dunlap. Her conclusions are essentially the same as his. She also fails to cite any research data in support of or in opposition to her conclusions.

There are, however, a number of research reports pertinent to this issue which have been available for some time and which it seems advisable to discuss briefly.

On the negative side of the issue there is a report by Parson (22), often referred to in the literature and recently cited by Bluemel (2) in an article dealing with stuttering and 'cerebral dominance.' Parson investigated the total public school enrollment of 13,000 pupils⁴ in Elizabeth, New Jersey, and concluded that in the four years the policy of training all left-handed children to write with the right hand had been in effect, not a single case of defective speech could be traced to the reversal of manual habits. He believed that whenever stuttering occurred as a result of changing the native handedness of young children, it lasted only while the change was being made. Of the 13,000 children, 250 were originally left-handed. Of this number, 66 remained left-handed. Parson's report is unfortunately fragmentary, being merely a small part of a chapter in his book *Lefthandedness*.

It is necessary to make at least four criticisms of Parson's study as reported. First, he does not describe the procedure followed in making the investigation. Because he reports to have investigated the total public school enrollment of 13,000 pupils, it is entirely possible that he relied upon questionnaire methods or on teachers' statements in regard to any information which he obtained. It is possible that more thorough

⁴ On page 102 of Parson's *Lefthandedness*, the school enrollment is given as 'nearly 13,000.' On page 103 it is given as 'about 15,000.'

procedures were followed but in the absence of any definite statement regarding such procedures as were followed, one is unable to evaluate properly the whole investigation.

Second, practically no details are given with regard to the procedure followed in changing handedness, or with regard to the success achieved in the attempt to change handedness. The fact that sixty-six of the left-handed children remained left-handed indicates not only that the policy of changing handedness was not completely carried out, but also it implies that the methods used for changing handedness were not adequate to affect a thorough-going reversal. If it were true that the shift of handedness involved only the acquisition of an unstated degree of skill in writing with the right hand, one would not be justified in assuming that the change of handedness was complete.

Third, Parson fails to state his criteria in arriving at his conclusion that no cases of stuttering could be traced to the change of handedness. His report does imply that there were some cases of stuttering involved, inasmuch as he refers to cases of stuttering in connection with his investigation. His final conclusions seem quite inadequate since his observations (made during a visit to Elizabeth⁶) concerned a period of only four years, and he does not state whether any cases of stuttering developed after this four-year period. It is to be assumed that the pupils enrolled in the three higher grades at the beginning of the study had left the schools before the end of the fourth year. They could not have been included in the final report unless they were observed after leaving school. We have no way of knowing whether any such follow-up observations were made.

Fourth, the handedness of the left-handed children was probably changed in accordance with the policy of the school superintendent. One is not justified in assuming that all of the individual teachers were competent to carry out this policy in an expert fashion, or that they were competent to evaluate

⁶ Parson says, "The series of manuscript tests in Elizabeth began on October 30, 1923, and continued during four days." *Lefthandedness*, page 104. Parson does not say whether he was in Elizabeth longer than four days. We are not here concerned with the manuscript tests.

the results of it, or that they possessed the courage to report definitely that in any case the change of handedness resulted in detrimental consequences.

In conclusion, it would seem pointless to advance Parson's report in support of or in opposition to any theory bearing on the question of relationship between stuttering and handedness.

A second negative report is made by Bluemel (2) who states that insurance companies and industrial boards report that claims for injury to the hand or arm are not associated with claims for accompanying speech disturbances. We may point out that if such claims are filed soon after the injuries, one would have no reason to expect mention of speech disturbances. Furthermore, these cases would be, probably, mature individuals whose speech would not be so easily upset by the enforced change of manual dexterity. Regardless of the actual facts in this connection, Bluemel's conclusion is rendered peculiarly inadequate by his disclosure that he has collected no statistics.

Wallin (33) made a study which has been cited as evidence against the contention that change of handedness may be associated with stuttering. In a questionnaire survey of 89,057 pupils in the St. Louis public schools, he found that the number of stutterers was 0.7 per cent of the total enrollment. Of the stutterers, 9.5 per cent were dextro-sinistrals,⁶ while only 2.0 per cent of the total population (which included the dextro-sinistral stutterers) were dextro-sinistrals. He found, also, that 3.6 per cent of all the boys had speech defects while only 2.0 per cent of all the girls had speech defects, but that there was no difference in the prevalence of speech defects as between male and female dextro-sinistrals. He reported, further, that 16.9 per cent of the colored dextro-sinistrals had speech defects. Of 27 dextro-sinistral stutterers for whom definite data were available, 18.6 per cent began to stutter after receiving instruction in writing in the schools. From these data, Wallin concluded, in a rather non-committal

⁶ Wallin defines a dextro-sinistral as a naturally left-handed person who has been taught to write with the right hand.

manner, that "the vast majority of our left-handed pupils who had been taught to write with the right hand had not developed any speech defects."

Regardless of Wallin's conclusion, the data which he presents indicate that speech defects, especially cases of stuttering, were more common among the pupils whose handedness had been changed than among the pupils whose handedness had not been changed. Four times as many of the dextro-sinistrals had speech defects, and more than four times as many of them were afflicted with stuttering, as compared with the total population (which also included the speech defective dextro-sinistrals). Six times as many of the colored dextro-sinistrals had speech defects, as compared with the total school population (which included the colored speech defective dextro-sinistrals). The fact that Wallin found no sex difference in the prevalence of speech defects among the dextro-sinistrals is highly important in view of the fact that he did find a considerable sex difference in the prevalence of speech defects among the general population. One would expect to find the same sex difference in both groups, if there were no relationship between the incidence of speech defects and the changing of handedness. In general, it is to be said that, even though Wallin found that the majority of pupils who were changed from the left hand to the right hand did not develop speech defects, he did find that a considerably larger proportion of them than of the other children did develop speech defects, and particularly stuttering. In fact, considering the crudeness of his methods, the findings are rather impressive.

From a series of studies, Ojemann (17, 18, 19, 20) concluded that most left-handed individuals who learn to write with the right hand do not develop speech disturbances. This conclusion was based, mainly, upon a study of twenty-three purely left-handed children who learned to write with the right hand. The twenty-three children were pure left-handers according to both the composite scores on his tests of handedness and the parents' judgments of handedness (18, p. 609; 20, p. 122). In no case did stuttering or any other

speech defect follow the enforced requirement of writing with the right hand. Four of the twenty-three children stuttered before school age. We have no criticism to make of Ojemann's fundamental conclusion, that requiring these left-handed children to write with the right hand did not produce stuttering. To a large degree, however, we feel that Ojemann's study is irrelevant to our issue as to whether a relatively complete shift of handedness is accompanied by stuttering in a significantly large number of individuals. On the basis of Ojemann's data, his cases are to be classified as fundamentally purely left-handed except that they wrote with the right hand. Moreover, the children were at least five or six years of age before the right-handed skill in writing was set up and it is reasonable to assume that this skill was not shifted from the left to the right hand, but was set up in the first place in the right hand. The child's native laterality was permitted its natural expression in essentially all the other manual activities.

What Ojemann has demonstrated is that in twenty-three purely left-handed children, who were required merely to learn the one skill of writing with the right hand, stuttering did not develop. Mindful of the many other research findings cited in this paper, we suggest that in these twenty-three cases the dominant lead control was not shifted from the left to the right side but that in a predominantly left-unilaterally controlled organism a single specific skill was established in the fundamentally non-dominant hand. It is interesting to note that when one subject at three years of age was forced by the mother 'to perform the principal unimanual activities' with the right hand, the child began to stutter. Upon the discontinuance of such training, "stuttering disappeared about one year and a half later, and did not appear again." It is extremely pertinent that no mention is made of any attempt to make such a thorough-going shift in any other case among the twenty-three cited by Ojemann.

We have discussed Ojemann's investigations at some length because they have been widely cited in opposition to the contention that a change of handedness is to be regarded as a factor in the etiology of stuttering. It is necessary to

make clear the very limited degree to which they are relevant to this contention, mainly because of Ojemann's selection of cases. Ojemann's findings are very valuable in indicating generally the degree to which handedness may be changed in some cases without producing stuttering. It must be remembered, however, that other studies, such as the one cited by Oates (16), have shown that in some cases even the shifting of a single skill, writing, to the non-dominant hand is followed by the onset of stuttering, so that it would be misleading to regard Ojemann's findings in twenty-three cases as being universally conclusive. Also, it is not to be overlooked that about seventeen per cent of Ojemann's cases gave a history of stuttering, as compared with one to two per cent among the general public school population. To dismiss these seventeen per cent by saying they revealed no relationship between stuttering and handedness condition (whether obvious change of handedness or some more basic phase of nervous system organization related to handedness) is to leave open the question of the interpretation of one of Ojemann's most striking findings. If it be coincidence that seventeen per cent of his twenty-three cases gave a history of stuttering, then one is challenged to discover the precise conditions under which such coincidence reveals itself.

In this connection, and for similar reasons, it is important to discuss an investigation by Koch (11). She made a detailed study of hand preference by noting the manual choices of about 200 college students in a large number and variety of standardized situations. Of the 200 students, forty-one mention a difficulty such as stammering, lisping, hesitating, etc.; and the number was increased to eighty-nine by including those subjects who claimed that they learned to talk at an age which, according to Gesell's standard, would indicate that their speech development had been somewhat retarded. She says, "Between the groups who do and do not report speech problems no reliable differences appear with respect to the relative frequency of individuals alleging themselves (1) to be left-handed at the time of taking the test or to have been at any time previous, or (2) to have had training in the use of the

right hand. . . . It is worthy of comment, though, that the Small Speech Difficulty Group, the group probably having in it the largest number of bona fide speech cases, has a slightly higher number of members who indicated that, having once been left-handed, they had been trained to use their right hand. Considering the probable weighting of our speech difficulty groups by a large number of non-speech cases, we may with some reason suspect the aforementioned differences of being valid." She reports, further, that the incidence of speech disorders seems to be commonly greater in the group showing both the failure of eye and hand preference to be in accord as to side, and frequent confusion in right-left discriminations. She concludes that reported irregularities or difficulties in speech seem not to be significantly related to a persisting or a cleared left-handedness, nor to failure of eye and hand preference to agree, but rather to confusion in discriminating right from left.

We anticipate that these conclusions might be interpreted as Ojemann's conclusions have been. For that reason, it is important to point out the high degree to which Koch's study is irrelevant to the question which we are discussing. It is irrelevant because out of a group of 200 college students, Koch selected eighty-nine individuals whom she regarded as having speech difficulties. It is extremely improbable that her speech defective groups actually included more than two or three stutterers in view of the fact that numerous surveys have shown that approximately one per cent of the population are stutterers. Koch does not state whether any of her subjects stuttered. The findings and conclusions of Koch, are pertinent only to the very broad question as to whether confusion or changes in handedness are related to speech disorders in general. It is to be emphasized that such investigations are not to be interpreted as having a direct bearing on the issue as to whether stuttering (not speech disorders in general) is related to confusion or changes in handedness.

Other investigations bearing upon the question under discussion have yielded more definitely positive findings. Two of these were made by Ballard (1). First, in a questionnaire

study of 13,189 children, he found that stuttering was approximately four times as frequent among dextro-sinistrals (those children who performed with the left hand all the common activities except those that they had been forced to acquire with the right hand) as among the rest of the children. In 944 mentally defective children the number of stutterers among the dextro-sinistrals was about twelve times as great as the number among non-dextro-sinistrals. In his second study, Ballard made a personal examination of each of 11,939 children. He found that among the dextro-sinistrals, seventeen per cent stuttered at present and twenty-five and eight-tenths per cent had stuttered.

Haefner (5) reported that of forty-one children who had been changed from left- to right-handedness, twenty-four per cent stuttered.

Of 263 stutterers having an average age of approximately 18 years studied at the University of Iowa, 30.4 per cent of the males and 26.3 per cent of the females reported that their original handedness (left) had been changed. Of 1,220 University of Iowa freshmen including stutterers, 5 per cent of the males and 3.3 per cent of the females reported that their original handedness (left) had been changed.

Milisen (13) made a detailed investigation of 8,000 children in the public schools of Council Bluffs, Iowa. He found that of this number (this included stutterers and former stutterers), 3.3 per cent had had their handedness changed. Of 116 stutterers, 20.7 per cent reported that their handedness had been changed. Of 85 pupils who had stuttered and who had apparently outgrown the disorder, 16.5 per cent reported that their handedness had been changed.

In a study of 33 adult stutterers, Fagan (4) reported that 27.2 per cent had been shifted from the left to the right hand.

Oates (16) refers to an experiment by Lewis who trained 12 subnormal right-handed children to use the left hand. In five months all 12 developed stuttering, and in all 12 the stuttering disappeared after the left-handed training was abandoned. This is pointedly interesting in view of Buford Johnson's remark, "If the right-handed child becomes skillful in a left-hand

activity, fear is rarely manifested over possible brain disturbances."

The studies here reviewed make necessary the conclusion that changes of peripheral handedness occur significantly more often among stutterers than among normal speakers.

III

The second issue for discussion in this paper is: Are stutterers, as a group, different from non-stutterers, as a group, with respect to peripheral sidedness?

Jasper (8) in a study of the neuromuscular organization of stutterers administered a laterality index test, devised by Wendell Johnson, to 332 University of Iowa sophomore students. This index was not 'sent out' as an ordinary questionnaire, but it was made a part of the regular laboratory work in a course in elementary psychology. The index was issued in a laboratory period with full instructions, both written and oral, as to how to fill it out and with emphatic instructions not to answer any item until the student had observed his own behavior for a week and had tried out every item to be answered. The index consisted of questions regarding seventy-six unimanual and eleven bimanual acts. From these 332 university students, Jasper selected three groups: thirty-five right-handed individuals, thirty-one left-handed individuals and thirty-nine ambidextrous individuals. These three groups were selected to give relatively pure cultures in regard to unimanual preference (right or left) and a lack of it (ambidexterity), as indicated by the index responses. For comparison with these normal speakers, Jasper had forty unselected adult stutterers. On the laterality index, the stuttering group matched the ambidextrous group almost perfectly.

Travis (25) in a study of the eyedness of 55 normal speakers and 48 stutterers found that for the first group 73 per cent were right-eyed, 22 per cent were left-eyed and 5 per cent were amphiocular. For the second group, he found that 50 per cent were right-eyed, 45 per cent were left-eyed and 5 per cent were amphiocular. Jasper (8) found for his right-handed group that 74 per cent of the cases were right-eyed, 23 per cent

were left-eyed and 3 per cent were amphiocular; for the left-handed group 45 per cent of the cases were right-eyed, 42 per cent were left-eyed and 13 per cent were amphiocular; for the ambidextrous group, 56 per cent were right-eyed, 26 per cent were left-eyed and 18 per cent were amphiocular; for the stutterers, 54 per cent were right-eyed, 21 per cent were left-eyed and 25 per cent were amphiocular. Both of these studies tend to classify stutterers with left-handed and ambidextrous normal speakers. Bryngelson (personal communication) reports that of 700 stutterers (463 males, 237 females), 57.3 per cent were left-eyed, 32.4 per cent were right-eyed and 10.3 per cent were amphiocular.

Mills (14), Parson (22) and Miles (12) report findings very similar to those of Travis and Jasper with regard to eyedness of normal speakers. They made no study of the eyedness of stutterers.

Milisen (13) found for 23 grade school stutterers that 50 per cent were right-eyed, 25 per cent were left-eyed and 25 per cent were amphiocular; for 28 grade school former stutterers, 83 per cent were right-eyed, 11 per cent were left-eyed and 6 per cent were amphiocular; and for 24 grade school children whose handedness had been changed from left to right, but who never developed stuttering, 50 per cent were right-eyed, 31 per cent were left-eyed and 19 per cent were amphiocular.

In a study of the simultaneous antitropic movements of the two hands (abduction and adduction), Travis and Herren (29) found the following to be true of the groups studied. Right-handed normal speakers with a history of right-handedness lead more frequently with the left hand; right-handed normal speakers with a history of left-handedness lead about as often with one hand as with the other; left-handed normal speakers with a history of left-handedness lead more frequently with the right hand; and right-handed stutterers, regardless of a positive or negative history of left-handedness, lead more frequently with the right hand.

In a study somewhat similar to that of Travis and Herren, Jasper (8) found that stutterers were to be classed with the left-handed normal speakers in that they had between 26 and 55 per cent more right than left leads. In order to avoid con-

fusion on the part of the reader, it should be pointed out that a right-handed lead on this test is characteristic of a left-handed individual and that a left-handed lead is characteristic of a right-handed individual. The reason for this is unknown. The important fact to be noted is that the stutterers tended to resemble the left-handed normal speakers rather than either the ambidextrous or the right-handed normal speakers on the basis of this test.

In a study somewhat related to the ones just cited, Orton and Travis (21) found that when the action currents from both forearms are recorded during simultaneous voluntary contraction, they may appear simultaneously in the two arms, but that in by far the greater number of trials those from one arm precede those from the other. In records of right-handed normal speakers the number of times that the action currents arrive first in the right arm exceeds such precedence in the left and also exceeds the incidence of simultaneous leads. In right-handed stutterers the greater number of leads is in the left arm and the number of simultaneous leads is much greater than in normal right-handed speakers.

Two studies have been reported on a comparison of the performances of stutterers and normal speakers in mirror tracing. Travis (25) found that in tracing a star in a mirror, first with the left hand and then with the right hand, 86 per cent of 48 stutterers did better with the left than with the right hand, while 14 per cent did better with the right hand than with the left hand. Of 55 right-handed normal speakers, 22 per cent did better with the left hand and 78 per cent did better with the right hand.

Jasper (8) made a detailed investigation of star tracing, and of his conclusions the following are most pertinent to the present discussion. First, on the basis of the average time required to trace the star with each hand, the stutterers were to be classed with the left-handed and ambidextrous normal speakers.⁷ The average time in seconds for the left hand was

⁷ The right-handed and ambidextrous normal speakers and the stutterers traced the star first with the left hand and then with the right, thereby giving the right hand the benefit of any practice effect from the first tracing. The left-handed normal speakers traced the star first with the right hand. See Table VIII, Jasper (8).

less than for the right hand among the stutterers and the left-handed and ambidextrous normals; and the average time was less for the right hand among the right-handed normals. A greater percentage of the stutterers than of any other group were better with the left hand. Jasper contends, with what appears to be good justification, that further research is needed before an adequate evaluation of the star tracing test can be made.

Of 176 unselected stutterers studied at the University of Iowa Speech Clinic, 61.9 per cent traced the star faster with the left hand than with the right hand when the star was traced with the left hand first.

Fagan (4) and Jasper (8) have reported studies on the comparative performances of normal speakers and stutterers in simultaneous handwriting. Fagan reported that 23 per cent of 69 normal speakers presented reversals with either one or the other of the two hands. None of the normal speakers reversed with both hands. Twenty-nine or 58 per cent of 50 stutterers presented reversals with either one hand or the other, or with both hands. Three of the 29 stutterers reversed with first one hand and then the other, or with both hands at the same time; 9 of the 29 stutterers reversed with the right hand only; and only 1 of the 29 stutterers reversed with the left hand only. Fagan concludes that stutterers show a significantly greater tendency to write mirrored script with either hand or with both hands than do right-handed normal speakers. Jasper found that a significantly larger percentage of the stutterers than of the pure right-handed and the pure left-handed normal individuals had reversals in simultaneous writing. The difference in the number of reversals between the stutterers and the ambidextrous normal speakers was not significant.

Travis and Lindsley (30) made an action current study of 20 pure right-handed and 20 pure left-handed normal speakers and 24 stutterers. All subjects were adults. They found that for all normal speakers except one the critical point (the amount of contraction beyond which the frequency of the action currents decreases with further increase in amount of

contraction) of the flexor digitorum sublimis muscle was less in the non-dominant than in the dominant arm. For 52 per cent of the stutterers, the critical point was less in the right than in the left arm, and for 13 per cent of the stutterers the critical point was the same for the two arms. This meant that for 65 per cent of the stutterers, either the left arm was dominant or the two arms were of equal dominance, on the basis of the critical point described above.

One of the most important studies related to this phase of our discussion was reported by Jasper (8). It concerned the study by the chronaxie technique of the relative excitability of homologous paired muscles. He found that 56 to 67 per cent of the group of right-handed normal speakers presented lower chronaxies on the right than on the left side for the biceps, the flexors of the digits and extensors of the digits. Thirty-eight to 50 per cent of the left-handed group of normal speakers showed lower chronaxies on the left than on the right side for these paired muscle groups. Fifty-two per cent of the stutterers gave no differences between the two sides for the biceps. Fifty-eight per cent gave no differences between the two sides for the flexors and 32 per cent gave no differences between the two sides for the extensors. The stutterers differed from all of the normal speakers in that with the latter group there was an average ratio of one to two between the biceps and the flexors of the digits, a ratio of one to two between the flexors and the extensors of the digits and a ratio of one to four between the biceps and the extensors. In the stutterers, there was an average of one to two between the biceps and the extensors, and there was very little difference between the biceps and the flexors. The average chronaxie value on the extensors for the stutterers was about one half of what it was on the same muscles for the normal speakers. On the whole, the results of this study by Jasper characterize the group of stutterers as having less unilateral and central-peripheral differentiation than normal speakers in the functional organization of the nervous system.

In general, with regard to peripheral laterality, stutterers appear to be more ambilateral than either right-handed or

left-handed normal speakers. The degree to which stutterers do exhibit unilaterality tends to classify them with the left-handed rather than the right-handed normal speakers.

IV

The third question pertinent to this discussion of stuttering and the concept of handedness is this: Are there differences in the hereditary backgrounds of stutterers and non-stutterers with respect to peripheral aspects of sidedness?

Bryngelson (26) reports that of 200 stutterers ranging in age from five to twenty-five years, 54 per cent had left-handed relatives in their immediate families. In an investigation in the public schools of Council Bluffs, Iowa, Milisen (13) used an unusually intensive case history technique and found that of 23 stutterers, 82 per cent reported left-handedness in their hereditary background and that of 28 former stutterers, 84 per cent reported left-handedness in their hereditary background. Of 168 unselected stutterers examined at the State University of Iowa Speech Clinic, 75 per cent had left-handedness in their hereditary background; 45.3 per cent reported left-handedness among parents and siblings.

Of 240 normal speakers, students at the State University of Iowa, 53 per cent reported left-handedness or ambidexterity, or both, in their hereditary background; but only 19 per cent reported left-handedness or ambidexterity, or both, among parents and siblings. These data were obtained by Milisen by essentially the same methods used in obtaining the heredity data from stutterers.

It appears, therefore, that there is more left-handedness in the hereditary backgrounds, particularly in the immediate families, of stutterers than of normal speakers.

V

The fourth issue for consideration in this paper is: Are stutterers, as a group, different from non-stutterers, as a group, with respect to the central neurological aspects of sidedness?

Probably the most significant study bearing on this particular phase of the discussion was done by Travis (24). He

recorded action currents simultaneously from the two masseter muscles during stuttering and during the free speech of both stutterers and normal speakers. In the free speech of both stutterers and normal speakers the two masseter muscles gave action currents which were very similar in frequency, extent, wave form and general patterning. During stuttering these muscles gave strikingly dissimilar action current patterns. The action currents from one side would be exceptionally large while those from the other side would be scarcely discernible; those from one side would appear in volleys at a rate at from ten to forty volleys per second while those from the other side would be absent, or greatly reduced in size, or continuous; the volleys from one side would appear out of time with respect to those from the other side; the action currents from one side would be of much greater frequency than those from the other side; and those from one side would give a pattern strikingly different from that shown by those from the other side. These findings indicate that during normal speech the two cerebral hemispheres are functioning concordantly and harmoniously to control the two sides of the speech mechanism as a unit, and that during stuttering they are functioning independently and disharmoniously to produce disconcertion of the speech act.

Other findings bearing on this issue were obtained by Jasper (8) in a study of the asymmetry of the perception of the phi-phenomenon between points of fixation and homonymous and heteronymous doublings. He found that for both far and near fixation the right-handed normal speaker experienced a consistent illusion of movement to the right; the left-handed normal speaker to the left; the ambidextrous normal speaker was inconsistent; and the stutterers perceived either no illusion of movement, or it was to the left, or it occurred both to the left and to the right. The most important interpretation of these facts is that they tend to indicate an ambidexterity or a left laterality of nervous organization in the stutterer. Jasper says: "These results seem to indicate that unilaterality in general neural organization is expressed in the field of perception as well as in the field of manual preference. The phi-

phenomenon test of both peripheral and central dominance clearly demonstrated the lack of unilaterality on the part of stutterers as compared with right and left-handed normal speakers, and it showed a tendency on the part of the stutterers to have more ambilaterality than the ambidextrous normal speakers. It differentiated further between the ambidextrous normal speakers and the stutterers in that the stutterers showed a tendency to more left peripheral dominance while the ambidextrous normal speakers showed a tendency to more right peripheral dominance."

In another study Jasper (8) compared the visual acuity of one eye with that of both eyes for both stutterers and normal speakers. He found mainly that the difference between the average acuity in monocular and binocular vision is significantly greater for all groups of normal speakers (left-handed, right-handed and ambidextrous) than it is for stutterers. The two eyes appear to function more in an additive manner in the binocular acuity of normal speakers than they do in stutterers. The exact meaning of this finding is unknown. However, it may be taken to indicate that in the case of the stutterer there is less likelihood of the synchronization of the two cerebral hemispheres in the functions involved in perception.

In still another study Jasper (8) determined the relative convergence strength of stutterers and normal speakers. The convergence strength test determined the ability of the subject to maintain the fixation of an object moved slowly toward the bridge of the nose. The stutterers were to be classed with the ambidextrous and left-handed normal speakers with regard to the relative convergence strength of the two eyes. Moreover, 20 per cent more of them than of left-handed subjects themselves presented a superiority of the left eye.

The most recent study bearing on this issue of our paper was made by Van Riper (32). By means of a test employing the simultaneous drawing with both hands of kinesthetic, visual, and script patterns on opposite sides of a vertical board he found that unselected stutterers, like ambidextrous normals, showed no consistent or typical trend either toward right-handed (characteristic of left-handed normals) or left-

handed (characteristic of right-handed normals) mirroring. Stutterers differed from all of the normal groups in frequently producing mirror patterns with both hands. It is difficult to explain these performances of stutterers in any terms save those implying a confused central laterality of nervous organization.

In general, these investigations point to the conclusion that in the stutterer, central nervous system organization is characterized by a high degree of ambilaterality, in contrast to the relatively high degree of unilaterality which characterizes central nervous system organization in the normal speaker.

VI

A number of studies have indicated that this ambilaterality of nervous organization in the stutterer is associated with a significant degree of neuromuscular disintegration. This disintegration has been observed in the form of transient disturbances in cortical function together with decrease in degree of cortical control over the functions of lower levels of the nervous system.

One avenue of approach to this phase of the problem is through the experimental investigation of certain tremors. It may be observed that the fingers, when extended, present small rhythmical movements or tremors which may be shown to occur at a general rate of eight to twelve tremors per second. Travis (31) found that in patients with the cortical motor pathways interrupted and the lower levels intact these tremors were not present. Further, he found that the tremors bear a definite relation to voluntary movements, a relation which would indicate that cortical activity is necessary for the appearance of the tremors. Starting from this point, Herren (7) determined that stuttering depressed the tremors at a rate of eight to twelve per second, and that voluntary movement of the finger produced a greater percentage of tremors of a rate of forty to seventy-five per second in stutterers than in normal speakers. To aid in the interpretation of these findings, Herren studied the effects of a depressing drug (alcohol) upon the tremors. He found that alcohol depressed the tremors at

a rate of eight to twelve per second and caused the appearance of two other tremor rates, one of fifteen to twenty tremors per second and the other of forty to seventy-five tremors a second. On the basis of Travis' findings and those of Herren on alcohol, one would tend to conclude that stuttering was coexistent with a reduction in cortical activity.

Travis (26) in an investigation of the patellar tendon reflex during stuttering obtained further data pertinent to this phase of the problem. Travis and Dorsey (27, 28) had found previously that when the activity is decreased in the highest levels in the central nervous system, the patellar reflex response latency was decreased; when the activity of the former was increased the patellar reflex response latency was increased. That is, the greater and lesser activity of the highest levels of the central nervous system respectively increased and decreased the reflex time. These relationships were determined for both human beings and dogs. Such results are not unexpected when one realizes that there is a constant relationship in the central nervous system between levels of the higher and lower order of development. It expresses itself in increased subservience of the peripheral arc when the higher levels are inordinately active and in decreased subservience of the peripheral arc when the higher levels are inordinately inactive. Increased and decreased subservience are denoted respectively by an increase and decrease of the patellar tendon reflex time. Thus, a decrease in the reflex response latency denotes a decrease in cortical activity or cortical dominance, and an increase in the reflex response latency denotes an increase in cortical activity or cortical dominance. From this standpoint, it was extremely significant for an understanding of the nature of stuttering for Travis (26) to find that during a tonic block the patellar tendon reflex time was significantly reduced in the majority of stutterers. Speech of the normal speaker, or normal speech of a stutterer, had no appreciable effect on the reflex time. During certain blocks of some stutterers, however, the reflex time was reduced as much as fifty per cent. As a general rule, during stuttering there was not only a reduction in the reflex response latency, but also a marked increase

in the extent and duration of the reflex action current volleys. In some of the stuttering records the action currents persisted twice as long as in the non-stuttering records. It appeared that the increase in the size and duration of the action currents and the markedly reduced reflex time were indications of the same thing, namely, a decreased subserviency of the lower levels which in turn was due to a decrease in cortical activity during stuttering.

Another investigation in point was that of Herren (6) who studied the effect of stuttering upon voluntary movements of parts of the body not directly concerned with speech production. He found that stuttering was often associated with the complete inhibition of rhythmical voluntary movements of the fingers. During an entire speech block the fingers would remain quiet. The longest of such periods occurred during a prolonged tonic spasm of the entire speech musculature. During clonic seizures the finger movements would cease for the first seizure and appear during the first release, cease again for the second seizure and appear again during the second release, and so on. Generally, one or two contractions were absent at a time. Usually, when both hands were used, the absence of movement occurred in the two hands at the same time. Sometimes, however, one hand continued to carry on the rhythm even after the other hand ceased to move. At certain times there would be a gradual decrease in the extent of the movements which would correspond roughly to the length and severity of the speech block. At other times stuttering frequently caused the two hands to change from opening and closing in positive phase to opening and closing in negative phase (when the one hand closed, the other hand opened, and vice versa). The same results were found in a study of the movements of the toes. It appears from this study that stuttering consists of tonic and clonic blocks not only in the speech mechanism, but also in extra-speech structures such as the hands and feet. Disturbances in voluntary movement of fingers and toes during stuttering is at least indicative of profound neuromuscular disintegration.

Jasper and Murray (9) found an automatic or reflex type

of eye movement during stuttering in oral reading that consisted of rapid movements and marked vertical twitches of the eye. These stood in striking contrast to the eye movements characteristic of normal speakers in oral reading and indicated that stuttering was associated with a temporary re-current reduction in the dynastic control of the superjacent levels of the central nervous system over their sub-structures.

Another study indicating the degree of neurological disintegration characterizing the stutterer was that of Murray (15). He found in stutterers a good many of the same disturbances in breathing during silent reading and "thinking" as occur during stuttering speech. The records indicated, among other things, abdominal inspiration and thoracic expiration at one time and abdominal expiration and thoracic inspiration at another time, and tonic and clonic spasms of the breathing muscles. Further, he found that during silent reading the stutterer commonly has more eye-movements per line of reading material than the normal speaker. Occasionally the stutterer begins to read from the right rather than from the left side of the page. His return eye-movements (from the right to the left side of the printed material) are less accurate and precise than are those of normal speakers. Instances of relatively long eye fixations appear frequently in the stutterer's reading. Perhaps these are to be thought of as analogous to tonic spasms in the speech muscles during stuttering.

SUMMARY

We discussed, first, in this paper the fundamental fact that an obvious relationship exists, neurologically, between handedness and the speech function. We cited particularly Weisenburg's comprehensive investigation of aphasia. We next pointed out the various aspects of the concept of handedness: (1) handedness in the sense of manual habits or usage; (2) handedness in the sense of manual preference; (3) peripheral handedness as distinguished from central nervous organization; (4) sidedness as distinguished from handedness.

We dealt, then, with four specific questions bearing directly upon the issue as to whether stuttering is related to handedness

or laterality. By means of critical evaluation of pertinent researches, we have been led logically to the following conclusions, corresponding to the four questions discussed:

1. Change of handedness occurs with significant frequency in the history of cases of stuttering. Stated differently, changes of handedness are followed by onset of stuttering in a significantly large number of cases. This is especially true when the changes of handedness are thorough-going. Almost all such changes are from left to right-handedness.

2. Stutterers differ significantly from right-handed normal speakers with respect to peripheral sidedness, showing a greater degree of left-laterality and ambilaterality. They differ from left-handed and ambidextrous normal speakers to a much smaller extent, differing least from ambidextrous normal speakers.

3. There is more left-handedness in the hereditary backgrounds of stutterers than of normal speakers; this is especially true with regard to left-handedness among parents and siblings.

4. The central, or cerebral, organization in stutterers is characterized by a high degree of ambilaterality, in contrast to the relatively high degree of unilaterality characteristic of the central, or cerebral organization in normal speakers. Moreover, this ambilaterality of nervous system organization in the stutterer appears to be associated with a significant degree of neuromuscular disintegration in the form of transient disturbances of cortical function, together with decrease in degree of cortical control over the functions of lower levels of the nervous system.

DISCUSSION

The above four conclusions have accrued from the widely scattered researches discussed in this paper. It is manifestly impossible to disregard the findings of these researches with impunity in any interpretation of stuttering. Any theory of stuttering which does not adequately account for these facts, regardless of how well it may account for other facts, is to that degree unacceptable. Statements such as those by Dunlap

and Buford Johnson, cited in the first part of this paper, to the effect that there is no relation between stuttering and changes of handedness as such, are explicitly and implicitly inconsistent with research reports which are and have for several years been available to all students of the subject.

We should like to point out, further, that the researches which we have cited do not indicate that innate left-handedness itself, the development of which is not interfered with, is causally related to stuttering. The present writers have never contended, and to the best of their knowledge no one familiar with pertinent and well-established facts has ever contended, that innate left-handedness stands in a causal relationship to stuttering. Nor do the pertinent facts give one any reason for contending that stutterers are generally left-handed in the sense that their manual habits are left-handed. There is reason to believe that on the basis of actual manual habits, however, they are generally more ambidextrous than are right-handed normal speakers.

The studies here reported indicate that perhaps the most important fact in connection with the general issue under discussion is that on the whole stutterers differ from normal speakers in being characterized by a relative lack of unilaterality of motor lead control. The factor of peripheral handedness constitutes only one phase of this condition. It is essential that a distinction be made between handedness and sidedness and between central and peripheral aspects of handedness and sidedness. In the light of the investigations reviewed, the functional organization of the central nervous system would appear to stand in a more significant relationship to stuttering than does peripheral handedness itself. It is reasonable to regard peripheral handedness as an indicator of certain aspects of central nervous organization, and as a means of affecting that organization to a limited degree.

In accepting the facts collected in this paper and in attempting to account for them in a theory of stuttering, one is in no sense bound to ignore other well established facts. There is need for emphasizing, however, that in acknowledging other well established facts one is in no sense justified in dis-

regarding the data cited in this paper. There is need, also, for the adequate regimentation of such other acceptable data regarding stuttering as may be available.

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THE ROLE OF VERBAL ACTIVITIES IN THE CONDITIONING OF HUMAN SUBJECTS

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After summarizing the experimental literature on conditioned withdrawal responses with shock as the conditioning stimulus in adult human subjects (8), Dr. G. H. S. Razran states that "while the withdrawal response to an electric shock is undoubtedly most useful in animal conditioning and of great interest in some special problems of human conditioning, its general value as a method for the discovery of the laws of conditioning in adult human beings is extremely limited." Dr. Razran's reviews of the experimental literature on the conditioned response (5, 6, 7, 8) show a degree of care and patience that is unusual, and they should be studied carefully by all who are interested in this subject. But after reading Dr. Razran's review of conditioned hand withdrawal responses in human subjects (8) and his review of conditioned responses in children (6), especially those parts of the latter which are concerned with conditioning the motor responses of older children, I have become more than ever convinced that the well known verbal activities, although more frequently neglected than not in the strictly behavioristic studies of the conditioned response, are somewhat central and basic in most of the conditioning of motor responses in human subjects. The present view of the role of verbal activities in the conditioning of human subjects has been influenced by the experience of carrying out an earlier experimental study in which an attempt was made to condition hand withdrawal responses in human subjects (2), and by the experimental investigations of Hamel (3) and Schilder (9).

In some of the conditioned hand withdrawal experiments, the movements of the hand in one direction have been ac-

curately registered by means of instruments, and several investigators have not felt badly about neglecting the implicit verbal activities of their subjects because these activities cannot be registered with the same degree of accuracy. The suggestion has even been made that any information about the subject's conscious thought processes is worse than no information at all. One may fairly express the sentiments of one group of experimenters in the following words: "Let us leave the conscious thought processes to the mentalists and the internal functions of the body to the physiologists and seek the general principles of externally observable behavior. And, furthermore, let us assume that the people who act as subjects for us are animals, or at least let us treat them as if they were animals."

It does not seem reasonable, however, to neglect the thinking and inner speech of human subjects just because the symbolic activities of subhuman animals are difficult to observe.¹ It is only on special occasions that human subjects have been treated as if they were subhuman animals, and the kind of subhuman animal selected for the occasion has not always been specified. The question might also be raised: if one is under obligation to treat people as if they were subhuman animals, why not treat all of the different subhuman animals alike? Should monkeys and rats be treated as if they were fishes and birds? Where should the line be drawn, or should any line be drawn? If the thought processes of people should be neglected because they are not found in rats, should we not also neglect those organic processes of monkeys which are not found in rats? It seems reasonable to believe that none of these organic processes should be neglected.

It appears that the suggestion of Bechterev (1) and Watson (11), that the conditioned hand withdrawal movement can take the place of the verbal report method in studying sensory discrimination in human subjects, is generally regarded as a mistake. It is even more generally recognized that one can pay proper attention to the role of verbal activities in motor

¹ Thinking in human subjects may include some organic processes that are not strictly verbal in nature (Watson, 12, pp. 202-215).

learning and still be a strict behaviorist (Watson, 12, pp. 180-215), although this attitude is not always taken in practise. The limitations of the verbal report method have been pointed out with considerable enthusiasm by some writers, but there are many organic activities which are by no means as accessible as inner speech. Verbal activities are generally conscious, but under ordinary circumstances one is also aware of what the hand is doing when a new act involving the hand is being learned. One is not conscious of all of the physiological processes that take place in any movement of the body. Movements of the hand may be involuntary, but inner speech and/or thinking are not entirely voluntary. Movements of the hand may acquire some of the characteristics of reflexes, as in reaction time experiments, but vocal and subvocal speech, in some people at least, is relatively automatic.

Several students of the conditioned response have had an aversion for the verbal report method or introspection because it seemed in one way or another to be related to 'consciousness,' but if such concepts as this one are laid aside there should be no objection to taking account of several organic processes that are present while learning takes place. Although it is not possible to study all of the simultaneous organic processes at once, it is not difficult to study more than one organic activity at a time. The usual custom is to attach the recording instruments to some convenient part of the body which is crucial and significant in the process being studied, so that the observations and results will have the maximum scientific value. I am inclined to think that when conditioned hand withdrawal movements of intelligent human subjects are being studied, it is a mistake to pay no attention at all to their implicit verbal activities.

When people have been treated as if they were animals and when no verbal reports have been asked for or permitted, the conditioned hand withdrawal responses with shock as the conditioning stimulus have on several occasions shown the most peculiar characteristics. A fairly large per cent of supposedly intelligent human subjects have not been able to learn to withdraw their hand when they heard a sound; and some

of these subjects have, for one reason or another, been excused from further efforts. Some of the more persistent experimenters were not able to teach their human subjects to move their hand when they heard a sound after more than one thousand repetitions of the training stimuli. Other experimenters, however, without seeming to put forth any unusual effort, have been able to teach their subjects to withdraw their hand when they heard a sound after only one repetition of the training stimuli. Several subjects have established this connection in themselves somewhat independently of the formal training. Conditioned hand withdrawal responses have been established in the hypnotic state more easily than in the waking state; normal and subnormal adults have shown only a slight difference in the efficiency of learning this response; and some of the subhuman animals have learned a withdrawal response more efficiently than human subjects. In Osipova's study of school children, with the sound of an electric bell as the conditioned stimulus and an electric shock as the conditioning stimulus (Razran, 6, pp. 60-64), the learning was more efficient in children under 14 than in older children, and it was more efficient in subnormal children than in normal children of the same age.² These results are somewhat surprising, and they cannot be readily explained by reference to the nervous connections with the hand alone. They suggest that there was some factor in the organism which the experimenters did not consider.³

The conditioned hand withdrawal responses which have been obtained do not have the speed and regularity of the withdrawal movements of the hand which are generally present in the simple reaction time experiment. The reason for this difference is apparently found in the fact that in the reaction time experiment, but not in the conditioned hand withdrawal experiment, the subject is told what he is supposed to do; and in some of the conditioned hand withdrawal experiments the subject may not know that he is supposed to do

² Workers in Bechterev's laboratory have used words or commands as conditioned stimuli with school children as subjects (Razran, 6, pp. 60, 71-73).

³ The explanations of these irregularities given by Bechterev (1, p. 356) and Razran (8, p. 140) are not entirely clear.

anything. When several hundred or a few thousand trials are made in the reaction time experiment, the hand withdrawal movement tends to acquire several of the characteristics of involuntary automatic acts. The well practised movement of the hand and arm in the reaction time experiment bears a close resemblance to a fairly large number of learned movements which appear to be quite reflex in nature, such as the individual acts involved in operating an automobile, and the individual movements involved in the well practised habits of sitting, standing, walking, dancing, chewing, swimming, typewriting, playing musical instruments, holding and carrying objects in the hand and under the arm, moving the hand towards one's hat when it is about to be blown off by the wind, jumping when a flash of lightning is seen, compensatory movements in falling, and moving the eyes along the lines of a printed page. Although many skilled acts become automatic, involuntary, and reflex in nature, the conditioned hand withdrawal movements acquire these characteristics only occasionally. Practically all of the responses of striped muscles which people learn can be learned more efficiently, and retained in a much more predictable manner, when the person is given the usual verbal information in regard to the act which he is to learn.

If the verbal activities of people were not closely connected with the conditioning of their motor responses, these comments on the importance of speech in conditioning would not be so pertinent, but the causal connection between verbal activities and the conditioning of motor responses is quite close. The desirability and in some cases the necessity of keeping the verbal processes in mind in connection with certain types of motor learning may be made more plausible by the three following examples.

1. If it is desired to form connections between several verbal stimuli and several movements of the right hand, the experimenter might say to the subject: "In your right hand, let the number of the little finger be one, the ring finger two, the middle finger three, the forefinger four, and the thumb five. Now when I speak a number from one to five, will

you tap on the table with the corresponding thumb or finger, but do not begin tapping until I finish speaking all of the numbers?" If the experimenter then says "12345432123454321," or some other similar and easily remembered series, the subject can tap out the appropriate combination on the table. In this illustration, the different verbal stimuli are given quite close together in time.

2. If the experimenter wishes to establish a connection between a present stimulus and a response which occurred ten days in the past, and which we shall assume has not occurred during the past ten days, the experimenter's instructions to the subject could take the following form. "When I speak the word 'dance,' will you execute the dance step which you showed me about ten days ago." If *E* then speaks the word 'dance,' *S* can execute the step. Or *E* could say, "When I say 'Beethoven,' will you play on the piano the musical composition which you played for us about ten days ago." If *E* then speaks the composer's name, *S* can play the selection on the piano.

In the two illustrations just given, the responses occurred ten days in the past, and the stimulus is given in the present; but it does not seem desirable to regard this very common type of learning as backward conditioning or as backward association, because the stimuli and responses overlap each other. The stimuli and the responses are concurrent in (a) learning, (b) retention, and (c) reproduction, for one does not have stimuli without responses or responses without stimuli. If the stimuli and the responses with which the stimuli are connected were not concurrent events, the one could not possibly have any causal influence on the other.

3. If *E* would like to form connections in *S* between present stimuli and future responses, he could give *S* the following instructions. "At 11:26 A.M. Friday week, begin writing poem *X* (which you already know) on the typewriter, and continue writing this poem for four minutes." *S* can use his own method of giving himself the necessary stimuli on the later occasion.

In this illustration, the verbal stimuli are given in the

present, and the motor responses occur in the future; but it does not seem desirable to regard this very common form of learning as a trace or successive conditioned response, partly because the responses in question would not occur in the future if some kind of stimuli were not present when or just before the responses occurred.

I think the reader will recognize that these three forms of learning, retention, and reproduction are frequently and widely used, and it would seem that our most general principles or laws of learning should, in some measure at least, take these forms of learning into account. Each of the three forms of learning described above has the following general characteristics:

1. Verbal activities are fundamental and basic in each of the three forms of learning, and by verbal activities we do not mean activities of the central nervous system alone.
2. No subhuman animal can be taught to do anything as complicated as the three acts described above in the same period of time, and no subhuman animal can be taught some of these acts in any period of time.
3. Intelligent human subjects can acquire the connections in each of the three forms of learning described above after only one repetition of the combined stimuli.
4. Since the formation of the connections requires in each of the three cases only one repetition of the combined stimuli, the alleged laws of 'use' and 'frequency' do not afford a plausible description of the learning process.
5. Since the acts learned in each of the three forms of learning described above may be pleasant, unpleasant, or indifferent, the pleasure-pain theory of learning does not afford an adequate description of the learning process.
6. The retention and reproduction of the acts or connections in each of the three kinds of learning described above are highly efficient and quite specific.
7. Finally, the fact that verbal activities are involved in each of the three forms of learning, retention, and reproduction described above seems on the whole to be more significant and basic than the existence of Gestalt patterns on some occasions.

Each of the three forms of learning described above is also on a higher and more complicated organic level than the simple type of the conditioned response.

When an intelligent person is told what he is to learn, he can readily form efficient associations between many different kinds of stimuli affecting his eye, or his ear, and the complicated responses of writing the answer with a pencil or pen, in some cases writing the answer on a typewriter, possibly writing the answer in any one of three or four foreign languages, or speaking the answer, thinking about it, recognizing it, etc. In cases of this kind the learning does not occur in the eye, in the ear, or in the hand, but largely in the sensory-neuro-motor verbal activities. Perhaps one should say that the learning occurs principally in the central nervous verbal activities, but the sensory and motor verbal activities are also necessary. These organic verbal activities of people are also central and basic in redintegrative learning, in serial learning, in the learning of the affectivities, in the learning of practically all kinds of skilled motor performances which involve the striped muscles, and also in the kind of learning situations which Professor Thorndike has used in most of his recent experiments (10). People learn better when they know they are supposed to learn something and when they know what it is they are supposed to learn.

In the present paper I have not claimed that verbal learning is the only kind of learning, and I have not assumed that the general principles or laws of learning, retention, and reproduction can be established best by a study of verbal learning, or even by a study of the learning processes of human subjects. Our principal suggestions are that some organic processes are more important than others in the learning processes of people, and that the analytical method of science requires that there should be a proper consideration of verbal activities in human motor learning. Verbal activities should be considered in spite of, or perhaps partly because of, the fact that they are conscious. Hudgins' experiment (4) in which the pupillary response of human subjects was conditioned to verbal stimuli is suggestive in this connection.

The human subjects in some of the conditioned hand withdrawal experiments have been carefully isolated from the experimenter; and they have been left alone with their thoughts. The excellent stimulating and recording apparatus which has sometimes been used has not overcome or eliminated the causal influence of the verbal activities. In studying conditioned hand withdrawal responses it does not seem a defensible scientific method to limit one's self to a behavioristic description of the hand itself when the psychological processes which afford a fairly satisfactory explanation of what the hand does are located elsewhere in the body.

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THE LIMITATIONS OF SCIENTIFIC PSYCHOLOGY AS AN APPLIED OR PRACTICAL SCIENCE

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In 1932 Klein¹ published a paper on a subject which is a matter of considerable concern not only to technical psychologists but also to the practical man and the more general student of human nature. At the beginning of his article Klein says, "Human nature as portrayed by the novelist is radically different from the psychology of the textbooks. The writer of so-called psychological novels may be utterly ignorant of the basic material introduced into our elementary courses and nevertheless handle his characters with a deftness and penetrating insight which causes us, in our non-professional moments, to agree that he 'understands' human nature. Using the term in this connotation it may be doubted whether the distinguished psychologists surpass the novelist in understanding man's behavior."

Klein makes some further statements which are of interest and ends with a fundamental question. "Rigorous training and exceptional competence in academic psychology may exercise little or no appreciable influence in the direction of making the student more expert in dealing with people. . . . It is not at all unusual for an alert police officer, a wide-awake business man, a successful physician, or a keen head-waiter to 'know' more about human nature than he does. And yet he is supposed to be the expert in this kind of nature. Is there something wrong with his training or are there two kinds of human nature: the kind described in our textbooks and the kind depicted in the novel or on the stage?"

These statements and questions challenge attention and thought. Klein finds that much of the trouble arises from the

¹ D. B. Klein, Scientific understanding in psychology, *PSYCHOL. REV.*, 1932, 39, 552-69. Quoted from page 552.

fact that psychology is rooted in the natural sciences whose methods and concepts are ill-adapted to the study of mental phenomena. The 'physico-chemical' descriptive category into which psychology finds itself so often forced is most unfortunate. Instead of this, Klein pleads for a 'program of courageous allegiance to the experiential phenomena *sui generis* irrespective of what metaphysicians may do with such phenomena.'

Most psychologists would probably agree with the notion that technical scientific psychology is woefully inadequate in answering the larger problems of human nature and of life. However, the solution which Klein offers is somewhat open to question. In this paper we shall take a different approach to the problem and shall reach a somewhat different conclusion than that arrived at by Klein.

We are speaking of textbook scientific psychology. We shall use the term 'scientific psychology' to designate any study of human nature in which there is a rigid control or an adequate control of the various conditioning factors which operate to bring about a given event or phenomenon. We shall maintain that laboratory psychology is the most 'scientific' form of psychology because in the laboratory the conditioning factors are most rigidly controlled. We shall further state and attempt to uphold an apparently paradoxical statement, namely, that the more scientific is any psychology the less practical it is, while, on the other hand, the less scientific, within wide limits, any psychology is, the more likely is it to have practical value. We shall maintain that there is no substitute for those generalizations involving percentages of cases which come from a wide experience with people by and at large. In other words, relatively unscientific 'field observation,' the method used by poet, novelist, business man, *et al.*, results in greater practical values than strictly 'scientific (laboratory) psychology.'

Every science started with what we may call 'field observation.' This type of observation has always proved inadequate for strictly scientific purposes for the simple reason that field observation does not permit of a careful control of

conditions. Chemistry and physics developed laboratories, that is, places where observations could be made under carefully controlled conditions. Psychology, too, has been driven to a laboratory for the same reason.

With the advent of the psychological laboratory and a more rigid control of conditions of the investigation there came about a necessary withdrawal from situations of everyday life. The life-situations were found to be too complex adequately to be observed and measured on a strictly scientific level. Psychology, as a rigid scientific study, gained much. It became able to isolate its variables and to control the conditioning factors to a considerable degree. On the other hand, psychology, as a scientific study, lost contact with vital problems of life. The extraordinarily well regulated and controlled conditions of the laboratory experiment could not be duplicated in practical life! Out of every carefully investigated problem in the laboratory came the discouraging (from a practical point of view) information that *it is difficult and dangerous to generalize in psychology!*

Some students of human nature have marched out from the laboratory and have attacked, with determination, some of the more complicated problems and questions of human nature, problems over which the scientific, laboratory psychologist would throw up his hands in despair. These people are our 'applied psychologists'. Often they have been looked upon askance by the more scientific psychologist and their work has often been designated as little more than pseudo-science. They have been mentioned as 'those who go where angels dare not tread.' On the other hand, the so-called practical or applied psychologist has often developed something of a return 'sneer' for the 'academic psychology' and has lost interest in strictly scientific psychology. This is an unfortunate condition because, as we shall show, these various workers all occupy a place on a 'scale of science-ness,' and all have a useful work to perform.

We have said that the scientific psychologist finds continually that he cannot generalize. As Carr² has recently pointed out, the quest for 'constants' in psychology is a vain

² Harvey Carr, The quest for constants, *PSYCHOL. REV.*, 1933, 40, 514-532.

quest. All that psychology can do is to point out the relations which exist between the many variables which are encountered, that is, to state the conditions under which any phenomenon or event takes place.

Let us take just one of hundreds of similar examples in psychology. One may read in some of the older texts that the whole method of learning is superior to the part method. Now what is the situation? In 1931 Grace McGeoch wrote as follows:³ "A critical analysis of the experimental data of 30 investigations . . . has suggested that the relative efficiency of the whole and part methods in learning and retention is a function of, at least, the following factors:

1. Subjects . . . age, training, memorizing ability and intelligence
2. Material . . . type, nature, difficulty and length
3. Practice . . . amount, and distribution and nature of practice
4. Form of the part method used
5. Method of measuring efficiency
6. Length of interval between learning and test

Experimental data, at the present time, do not justify any generalization regarding the specific mutual effects of these factors."

The above statement is typical. One finds that event or phenomenon *X* results when conditioning factors *A, B, C, D, E, F, G, H, and I* are present. The only 'generalization' which can be made is that *X occurs under these specific conditions*. Change any one of these factors and *X* is changed, that is, it is *X* no longer. As psychological investigation proceeds we shall have more and more of this analysis and statement of conditioning factors. The truly scientific textbook of the future will bristle with these masses of detailed statements of conditioning factors. I doubt if the book will be of much interest to the student and I am sure that it will be of next to no interest to the layman who wishes to deal with large practical problems of human nature. But it will be scientific in the most strict sense of the term!

³ Grace O. McGeoch, *Psychol. Bull.*, 1931, 28, 713-39. Quoted from page 737-38.

We have now arrived at the crux of the whole difficulty in making psychology a practical or applied science. Scientific psychology cannot generalize beyond its specific sets of conditioning factors to general complicated life-situations. *Human nature is very complicated*, some over-simplifying psychologists to the contrary notwithstanding! The 'set-up' of conditions with which physics and chemistry are concerned is much simpler, and for that reason these sciences have great general applicability to life-situations.

Even if we shift our whole problem of investigation so that we give up physical and chemical explanations for mental states and behavioral phenomena, as Klein suggests, we are not at all certain that the situation will be much improved. To take huge, vague mass-phenomena and account for them in terms of other huge, vague mass-phenomena or general conditions will lead to little of substantial knowledge. This would be a kind of super-Gestalt approach, but sooner or later the prying eye of the investigator would likely find essential parts of the mass which would have to be isolated and considered.

The writer can see no other conclusion than that scientific psychology is and must be of little *practical value*. At the same time the writer is of the opinion that some of the less scientific psychology is of considerable practical significance. We cannot expect a training in scientific experimental psychology to fit one to understand human nature in general. The more scientific the psychologist becomes the more must he retire from the general and complicated problems to more restricted problems and work in isolation from the world at large. There is simply nothing that can be done about the matter. However, the scientific psychologist might be more tolerant of other students of human nature (practical psychologists, novelists, and the like) because much of their knowledge stands the pragmatic test. On the other hand the applied and practical psychologist should be keenly aware of the fact that his work is, at best, only loosely scientific in the strict sense of the term.

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